

Small and Large Firms over the Business Cycle[†]

By NICOLAS CROUZET AND NEIL R. MEHROTRA*

This paper uses new confidential Census data to revisit the relationship between firm size, cyclicalities, and financial frictions. First, we find that large firms (the top 1 percent by size) are less cyclically sensitive than the rest. Second, high and rising concentration implies that the higher cyclicalities of the bottom 99 percent of firms only has a modest impact on aggregate fluctuations. Third, differences in cyclicalities are not simply explained by financing, and in fact appear largely unrelated to proxies for financial strength. We instead provide evidence for an alternative mechanism based on the industry scope of the very largest firms. (JEL D22, E32, G32, L25)

An important line of research in macroeconomics and corporate finance documents cross-sectional differences in the response of firms to aggregate shocks. Following the work of Gertler and Gilchrist (1994), this literature has paid close attention to firm size. This focus was motivated by the idea that, since size may proxy for financial constraints, a greater sensitivity of small firms to the cycle would provide evidence in favor of the “financial accelerator”—the view that financial frictions can amplify the response of the economy to aggregate shocks.¹ However, largely because of data limitations, vigorous debate remains as to both the basic facts and their financial interpretation. More generally, relatively little is known about systematic differences in sensitivity of firms to the business cycle.

*Crouzet: Kellogg School of Management, Northwestern University (email: n-crouzet@kellogg.northwestern.edu); Mehrotra: Federal Reserve Bank of New York (email: neil.mehrotra@ny.frb.org). Gita Gopinath was the coeditor for this article. The views expressed in this paper are those of the authors and do not necessarily represent the view of the US Census Bureau, the Federal Reserve Bank of New York, or the Federal Reserve System. All findings have been reviewed to ensure that no confidential information is disclosed. We thank three anonymous referees, seminar and conference participants at Barcelona GSE Summer Forum, Boston College, Boston University, Brown University, Chicago Booth, Colby College, Duke Finance, the Federal Reserve Board, Imperial College, the Inter-American Development Bank, the Minneapolis Fed, Notre Dame, Northwestern Kellogg, Society for Economic Dynamics, NBER Monetary Economics, NBER SI Capital Markets, NBER SI EFCE, Minnesota Macro, the Philadelphia Fed, Pittsburgh/Carnegie Mellon, the University of North Carolina, UCLA, and Wharton for useful comments. We thank our formal discussants: Paco Buera, Simon Gilchrist, John Haltiwanger, Isabelle Méjean, Morten Ravn, and Vincenzo Quadrini. We also thank Andy Atkeson, Manuel Amador, Cristina Arellano, Scott Baker, Effi Benmelech, V.V. Chari, Larry Christiano, Anna Cieslak, Janice Eberly, Mark Gertler, Kyle Herkenhoff, Ben Iversen, Patrick Kehoe, Ellen McGrattan, Fabrizio Perri, Mitchell Petersen, Ben Pugsley, Adriano Rampini, Ricardo Reis, Jón Steinsson, Julia Thomas, Johannes Wieland, and Thomas Winberry for helpful discussions and useful suggestions. We thank Apoorv Gupta, Alice Jun, and Adriana Troiano for excellent research assistance.

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¹The view that financial frictions may be responsible for the greater sensitivity of small firms to recessions is buttressed by an extensive corporate finance literature in which private and bank-dependent firms are often treated as being more financially constrained, which we discuss in Section I.

In this paper, we bring new evidence to bear on these issues. We address three questions. First, are small firms more cyclically sensitive than large firms, and if so, to what extent? Second, what would happen to aggregate fluctuations if the sensitivity of small firms matched that of large firms? Third, is this greater sensitivity a manifestation of differences in access to financing?

Our new evidence comes from the confidential microdata underlying the US Census Bureau's Quarterly Financial Report (QFR), a survey that collects income statements and balance sheets of manufacturing, retail, and wholesale trade firms. The QFR uniquely provides balance-sheet and income-statement data for smaller, private firms over a long period; a priori, this is a set of firms that one expects to be most financially constrained. We use QFR micro records to assemble a representative, quarterly panel of US manufacturing firms from 1977 to 2014. The resulting dataset is made up of approximately 1.1 million observations on 90,000 different firms. We use this dataset to quantify the greater sensitivity of firms at the bottom of the size distribution, relate it to the behavior of aggregate quantities, and assess whether it is evidence of a financial amplification mechanism.

To our knowledge, this paper is the first to use this firm-level data in its panel format. In contrast to the public releases of the QFR, the microdata allows us to accurately measure the magnitude of differences in cyclicity by firm size and to introduce firm level controls to determine the financial or nonfinancial factors that drive the size effect. As we detail in the paper, the existing literature that relies on the public releases has disagreed on the former and cannot address the latter.² Finally, the firm level data allows us to determine whether any average differences across firm size are statistically significant.

Using the QFR microdata, we find evidence of greater cyclical sensitivity among small firms. On average over the sample, the difference between sales growth of the bottom 99 percent of firms and the top 1 percent of firms (by book assets) exhibits a strong contemporaneous correlation with GDP. Our baseline estimate is that a 1 percent drop in GDP is associated with a 2.5 percent drop in sales at the top 1 percent of firms and a 3.1 percent drop in sales in the bottom 99 percent. The size asymmetry also appears in firm level regressions that control for industry and disaggregate firms into finer size quantiles. We adopt this stark notion of small and large because of the absence of measurable differences within the bottom 99 percent.

The size effect is concentrated at the very top of the distribution—the top 0.5 percent of firms; variation in elasticity of sales to GDP outside of the top 0.5 percent is small and statistically insignificant. In and of itself, the wide range of firm size with no measurable size differences in cyclicity suggests that financial factors may not account for the size effect. Firm size in our data ranges from less than \$200K in assets for the smallest firms to \$750 million (real 2009 dollars) in assets for firms in the ninety-ninth percentile; it is not obvious that financial frictions should be similarly severe over such a wide range of firm size.

²Estimates of the higher cyclicity of small firms range from small firms being approximately twice as responsive to monetary shocks as large firms (Gertler and Gilchrist 1994), to being equally responsive to recessions (Chari, Christiano, and Kehoe 2013; Kudlyak and Sanchez 2017), to being significantly less responsive (Moscarini and Postel-Vinay 2012).

The greater sensitivity we uncover for sales growth at small firms also holds for inventory growth and investment rates. As with sales growth, this differential is concentrated at the top 0.5 percent of the asset distribution. Additionally, we show that these results survive a large battery of robustness tests and that they also hold in the retail and wholesale trade portions of the QFR sample. Finally, we compare our results to prior work on differences in cyclicalities across size groups, in particular, we show how growth rates derived from the microdata deliver consistent and stable estimates of the size effect, improving on the previous literature.

We find that the greater sensitivity of the bottom 99 percent of firms, although statistically significant, is too small in magnitude to have an effect on the cyclical behavior of aggregates. Our data allows us to construct counterfactual paths for aggregate sales growth, inventory growth, and investment under the alternative assumption that cyclical sensitivities are the same in the cross-section and plot these counterfactuals against realized aggregate sales growth. The difference (seen in Figure 3) is negligible. This finding is due to combination of extreme skewness of the distribution of sales and investment in the cross-section and absence of sizeable differences in cyclicalities. For instance, the top 1 percent of firms accounts for approximately 75 percent of total sales and 85 percent of total investment in the latter parts of the sample. Moreover, this concentration has been rising over the last 30 years.³

Our findings verifying the greater cyclicalities of small firms beg the question of whether these differences in cyclicalities are driven by a financial accelerator mechanism. Gertler and Gilchrist (1994) argued that size serves as a proxy for the degree of financial constraints given that small firms exhibit greater bank dependence, cannot issue debt publicly, and face greater idiosyncratic risk. We verify that it is indeed the case that small firms differ from large firms along these dimensions.⁴ However, we provide three findings that cast doubt on whether the size effect is evidence of a financial accelerator mechanism.

First, we introduce direct controls for balance sheet ratios emphasized in the financial frictions literature that should affect the cost of external financing. We sort firms into leverage, liquidity, and bank dependence categories. We also introduce dummies for whether a firm has accessed public debt markets in the past and whether it recently issued dividends. We find that none of these controls eliminates the size effect; additionally, the quantitative magnitude of the size differential is almost unchanged. Ex ante, one would have expected these variables to explain at least some of the size effect; the fact that they do not is surprising and an indication that the size effect may not be due to financial frictions.

Second, to address the possibility that size is simply a better proxy for financing constraints than other balance sheet variables, we examine whether firm leverage behaves differently for small and large firms. A typical prediction of financial accelerator mechanisms is that the supply of credit to financially constrained firms

³This rise in concentration mirrors the findings of Autor et al. (2017), though we find that rising sales concentration in manufacturing comes in two waves (the early 1980s and late 1990s). Our findings with respect to skewness also echo Gabaix (2011), but we nevertheless find that cyclical fluctuations at the “median” firm (which is too small to affect aggregates) correlates strongly with aggregate fluctuations.

⁴However, importantly, these average differences in capital structure across size groups are dwarfed by heterogeneity in capital structure within each size group.

should be more cyclically sensitive. Thus, external financial flows (in particular, net debt flows) should show a higher responsiveness to aggregate conditions among financially constrained firms.⁵ We test this prediction using a simple event study framework around the recession dates in our sample. We find a statistically significant difference in the response of sales and investment across size groups, but no such difference in the response of debt. Total debt, bank debt, and short-term debt all behave very similarly among small and large firms.

Third, we investigate the size-dependent responses of investment and debt flows to identified monetary policy shocks. Arguably, the financial accelerator mechanism may be more acute in response to monetary policy shocks as they impact firms' cost of capital more directly. Using the method from Jordà (2005), we project firm-level responses of sales and investment on the identified monetary policy shock series of Romer and Romer (2004) (extended by Wieland and Yang 2020 up to 2007). Results from this approach are qualitatively consistent with the findings of Gertler and Gilchrist (1994) with small firms more responsive to the shock, but lack statistical significance for most dependent variables with the exception of inventories. Additionally, we find no evidence that bank debt or short-term debt contract faster at small versus large firms after monetary policy shocks. Overall, neither the regression evidence, nor the behavior of debt, nor the differential responsiveness to monetary policy shocks provides strong support in favor of the view that the size effect reflects financial constraints.

Given the absence of compelling evidence in favor of financial amplification, we also search for nonfinancial explanations for the size effect. We merge the QFR with establishment-level data from Dun and Bradstreet and construct firm-level measures of industry scope of firms—the number of distinct industries in which a firm's establishments operate. Industry scope is correlated with size, but there remains substantial variation in industry scope among the largest firms. Crucially, when simultaneously controlling for size and industry scope, we find that differences in cyclicity by size disappear. This result is robust to adding other controls, including the total number of establishments belonging to a firm, and they hold both in the manufacturing and trade samples. We consider a simple model in which firms can make their demand less elastic by investing in customer capital and enjoy economies of scope in making this investment across multiple industries. Our model makes multi-industry firms larger in equilibrium and less sensitive to aggregate fluctuations, providing a parsimonious, nonfinancial mechanism that accounts for our empirical findings.⁶

The remainder of the paper is organized as follows. Section I discusses how our evidence informs theories of the financial transmission of aggregate shocks and provides some caveats for our findings. Section II details the construction of the QFR dataset and provides summary statistics for small and large firms. Section III provides time series and regression evidence on the response of small and large firms over the business cycle and in recessions. Section IV analyzes the aggregate

⁵We illustrate this mechanism in a model in which firms differ by size and firm size is perfectly correlated with a binding financial constraint; the model is described and analyzed in online Appendix A.

⁶It should be noted that the ability to diversify in the past may itself reflect financial factors. We thank an anonymous referee for pointing this out.

implications of size asymmetries between small and large firms. Section V presents findings on whether the size differences we document are evidence of a financial accelerator, including the effect of identified monetary policy shocks. Section VI proposes a nonfinancial explanation for the size effect and presents supporting empirical evidence. Section VII concludes.

I. Contribution and Caveats

Why is the evidence in this paper useful?—This paper tests two propositions: (i) small firms are more cyclically sensitive than large firms; (ii) this difference is due to financial frictions. Our contribution is to show that while there is evidence of the former, our data shows very little evidence of the latter. Why are these findings meaningful, and how do they inform theories of financial transmission of shocks to firms?

The two propositions that we test were the focus of an early empirical literature on the financial accelerator. The seminal theoretical contributions of Bernanke and Gertler (1989) and Bernanke, Gertler, and Gilchrist (1999) show how aggregate shocks can be amplified by procyclical movements in credit supply. This insight led to an extensive literature seeking evidence of this mechanism. Though their models did not, strictly speaking, feature firm heterogeneity, the early empirical literature chose to focus on cross-sectional tests following a “difference-in-difference” intuition that, if the financial accelerator is operative, then financially constrained firms should be more responsive to aggregate shocks.⁷ The form of cross-sectional heterogeneity that this literature explored was often size, as it was generally accepted as providing a good proxy for the degree of financial frictions.⁸ The most influential contribution in this literature is Gertler and Gilchrist (1994), who show that sales and investment of small firms respond more to monetary policy shocks, but other early influential examples include Sharpe (1994) (employment cyclicality by size), Gilchrist and Himmelberg (1995) (cash flow shocks by size), and Oliner and Rudebusch (1996) (response of financing to monetary policy shocks by size).

Since then, another literature in macroeconomics and corporate finance has developed and analyzed models with heterogeneous firms and financial frictions which can be more closely compared to the cross-sectional evidence described above.⁹ Our evidence can be useful in evaluating models that deliver the joint prediction that size is correlated with the severity of financial frictions and that more constrained firms are more cyclically sensitive.¹⁰

⁷Summarizing the theory, Bernanke, Gertler, and Gilchrist (1996, p.1) write, “[A]t the onset of a recession, borrowers facing high agency costs should receive a relatively lower share of credit extended (the flight to quality) and hence should account for a proportionally greater part of the decline in economic activity.”

⁸For instance, Gertler and Gilchrist (1994, pp. 312–13) argue, “While size per se may not be a direct determinant, it is strongly correlated with the primitive factors that do matter ... [s]maller firms rely heavily on intermediary credit while large firms make far greater use of direct credit, including equity, public debt, and commercial paper.”

⁹A non-exhaustive list of important contributions includes Whited (1992), Cooley and Quadrini (2001), Gomes (2001), Hennessy and Whited (2005), Cooley and Quadrini (2006), Hennessy and Whited (2007), Khan and Thomas (2013), Moll (2014), Buera and Moll (2015), Gopinath et al. (2017), Ottonello and Winberry (2017), Buera and Karmakar (2017), Zetlin-Jones and Shourideh (2017), Begenau and Salomao (2018), and Mehrotra and Sergeyev (2020).

¹⁰Examples of macro models that generate the joint prediction we test include Cooley and Quadrini (2006); Khan and Thomas (2013); Buera, Fattal Jaef, and Shin (2015); and Mehrotra and Sergeyev (2020). These models

To be clear, as we show in online Appendix A, financial amplification at small firms is not a robust prediction of all heterogeneous firm models with financial constraints. What features must a heterogeneous firm model have to deliver amplification at small firms? Details depend on the particular model, but we argue that models where *ex ante* heterogeneity is generated by net worth and financing constraints are strongly procyclical will generate financial amplification at small firms.¹¹ Our evidence therefore rejects models where heterogeneity is driven by net worth or financing constraints are strongly procyclical.

Aside from the theoretical literature, our evidence also highlights the pitfalls of using differential responses by firm size as a way to diagnose the presence of a financial amplification channel in empirical work. Aside from the early literature cited above, more recent work on credit shocks in the Great Recession, for example by Mian and Sufi (2014) and Chodorow-Reich (2014), uses the absence or presence of differences across firm size as tests for a financial amplification channel.¹² Recent empirical work by Chaney, Sraer, and Thesmar (2012); Siemer (2019); Duygan-Bump, Levkov, and Montoriol-Garriga (2015); and Zwick and Mahon (2017) are also representative of how differential responses to shocks across firm size are used as evidence for financial amplification.

Our evidence also challenges the conventional wisdom on two other issues: the contribution of small firms to cyclical fluctuations and the view that financial amplification should be most prominent among privately traded firms.

The empirical literature on the financial accelerator made the case that the cross-sectional effects it finds contribute meaningfully to aggregate fluctuations. Fazzari, Hubbard, and Petersen (1988) argue that “financing constraints could account for a large proportion of the aggregate variability of investment.” Kashyap, Lamont and Stein (1994) argued in a similar vein about the effect of financial constraints on inventory investment in the 1981–1982 recession. Gertler and Gilchrist (1994) argue that small firms account for up to 60 percent of the total response of sales to monetary policy shocks (a finding we discuss in more detail in Section IVD). More recently, Cloyne et al. (2018) argue that, in US data, the response of young firms makes up two-thirds of the total firm investment response of publicly traded firms to monetary policy shocks. Dinlersoz et al. (2018) argues that private, leveraged firms (likely to be smaller firms) contribute substantially to the decline in sales in the Great Recession.

show that financial shocks elicit a stronger response of employment at small firms relative to large firms. In the corporate finance literature, Hennessy and Whited (2007) estimate stronger financial frictions in small relative to large firms while Begenau and Salomao (2018) examine the cyclical of equity and debt payouts by firm size in a model where small firms are more likely to be constrained.

¹¹Persistent differences in productivity may mean large firms are constrained while small firms are not (Cooley and Quadrini 2001, Mehrotra and Sergeyev 2019). The slope of the credit supply curve and its response to aggregate shocks is also key; constrained firms that operate on a more inelastic portion of their credit supply curve may actually respond less to shocks (Ottonello and Winberry 2017, Buera and Karmakar 2017).

¹²Specifically, Mian and Sufi (2014, p. 2211) argue that the channel through which housing net worth lowers employment is through demand rather than tightening credit supply, using size to rule out this possibility: “If our main result were driven by credit supply tightening, then we would expect the result to be stronger among smaller establishments that are more likely to be credit-constrained.” Chodorow-Reich (2014, p. 5) relies primarily on banking relationships with Lehman to identify financially constrained firms but uses the differential sensitivity by size as further validation for the credit supply effects of the Lehman bankruptcy: “The finding of differential effects at large and small firms can serve as a specification check for the validity of the research design.”

Unlike studies that rely on public firm datasets like Compustat, the QFR allows for inferences about the role of small firms in aggregate fluctuations. Our results indicate that the higher cyclicality of small firms, while present, is generally not sufficiently large to meaningfully amplify aggregate fluctuations. It is important to note that this is not a foregone conclusion; it depends both on the fact that small firms contribute a small and declining share to aggregates and their cyclicality to be fairly close, in absolute terms, to that of large firms.

The view that financial accelerator effects would be most prominent in a dataset of nontraded, nonpublic firms has been articulated repeatedly in the literature.¹³ More recently, Kudlyak and Sanchez (2017) allude to the advantage of firm-level QFR data.¹⁴ Much of this literature assumed that firm-level data on nonpublic firms would most strongly demonstrate the presence of a financial accelerator; we find that this is not the case.

What the Evidence in This Paper Does Not Say.—Before proceeding, we provide some cautionary notes for interpreting our findings. The absence of a size effect does not (i) imply the absence of a correspondence between firm size and access to external financing; (ii) imply the generalized absence of a financial accelerator mechanism; (iii) contradict evidence on the effects of financial frictions on employment in the Great Recession.

We cannot reject the view that firm size may be an important determinant of access to financing. Section IID shows that the composition of leverage does vary across firm size, with smaller firms relying more heavily on bank debt and on short-term debt, which may reflect the presence of financing constraints. There persists an ongoing debate in the corporate finance literature over the best empirical proxies for measuring financial constraints at the firm level, and the relevance of size in particular.¹⁵ However, our results indicate that, if these constraints are present, they do not amplify the sales and investment fluctuations of small firms. Moreover, as we noted earlier, as a theoretical matter, firms may be simultaneously constrained but display no differential response to aggregate shocks by virtue of being constrained.

Additionally, our evidence should not be taken as implying that the financial accelerator mechanism is not operative; it may simply be the case that firm size is a poor proxy for financing constraints.¹⁶ Online Appendix C explores this question in more

¹³Bernanke, Gertler, and Gilchrist (1996) and Kashyap, Lamont, and Stein (1994) explicitly cited the advantages of a dataset of private firms. Kashyap, Lamont, and Stein (1994, p. 574) stated, “Ideally, we would prefer to also examine nontraded firms, since we suspect that these companies are most dependent on bank financing and hence most likely to be susceptible to a credit crunch. Unfortunately, we are unaware of any consistent firm-level data for nontraded companies.”

¹⁴Specifically, Kudlyak and Sanchez (2017, p. 67) write, “The publicly available QFR data used in the analysis are available in an aggregated form by a nominal asset class. Consequently, the data do not allow splitting the firms by other characteristics. We thus use the Compustat data . . .”

¹⁵Size is often used alone or as part of an index as a proxy for financial constraints; see, among many other examples, Rajan and Zingales (1995) and Almeida, Campello, and Weisbach (2004). Recently, general indices of financial constraints derived from structural models and computable using observable balance sheet data have been proposed by Whited and Wu (2006) and Hadlock and Pierce (2010), for example. These indices typically rely, at least in part, on firm size. More recently, Farre-Mensa and Ljungqvist (2016) question the validity of a host of measures including Whited-Wu and Hadlock-Pierce, based on a novel test examining the responsiveness of firm leverage to changes in state corporate tax rates.

¹⁶Our data are silent about the importance of financial frictions for firm growth and innovation in the medium and long-run because of the rotating panel structure. In particular, recessions may have long-run scarring effects due

detail by looking at sales, investment, and external financing in recessions for firms sorted along dimensions other than size. For most of the proxies for financial constraints used in this paper, these differences are insignificant. However, for dividend issuance, we find substantial differences in the behavior of investment during recessions. Our objective is to establish that size differences do not support the financial accelerator mechanism.

Finally, an important literature shows that *employment* contracted faster at firms that are identified as financially constrained (see Chodorow-Reich 2014 and Duygan-Bump, Levkov, and Montoriol-Garriga 2015). Our data does not feature employment making it difficult to pinpoint the differences. In Section IV, we establish that, even if one finds modest differential cyclicality for employment by firm size, these differences will be more relevant for aggregate fluctuations in employment given the lower degree of skewness relative to sales or investment.

II. Data

A. *The Quarterly Financial Report*

The Quarterly Financial Report (QFR) is a survey of firms conducted each quarter by the US Census Bureau (US Census Bureau 2020c). The survey covers several sectors of the US economy: manufacturing, mining, wholesale and retail trade firms. Surveyed firms are required to report an income statement and a balance sheet each quarter. Data collected by the QFR is used as an input in estimates of corporate profits for the national income and product accounts, as well as in various other official statistical publications, such as the Flow of Funds.¹⁷

The QFR data is a stratified random sample. This sample is created using corporate income tax records provided by the Internal Revenue Service (IRS) to the Census Bureau. Any manufacturing firm that files a corporate income tax return (Form 1120 or 1120-S) with assets over \$250K may be included in the QFR manufacturing sample. For other industries, the inclusion threshold is \$50 million; for this reason, most of the analysis of this paper will be conducted using the manufacturing sample. The random stratification is done by size, meaning that firms above certain size thresholds are included in the QFR sample with certainty, whereas smaller firms are sampled randomly. Since 1982, firms with more than \$250 million in book assets are sampled with certainty; the microdata therefore includes the universe of such firms. Firms with less than \$250 million in assets are instead sampled randomly, so that the microdata contains only a representative sample. Each quarter and for each sector, a set of firms with less than \$250 million in book assets is randomly drawn and included in the sample for the following eight quarters. At the same time, approximately one-eighth of the existing sample stops being surveyed. For firms with less than \$250 million dollars, the microdata is thus a rotating panel, akin to the Current Population Survey (CPS). In manufacturing, the exact coverage of the sample relative to the population of firms varies across quarters,

to diminished firm entry (see Siemer 2019, Moreira 2016, and Alon et al. 2018).

¹⁷The QFR has its origins in World War II as part of the Office of Price Administration. The survey was administered by the Federal Trade Commission until 1982, when it was transferred to the Census Bureau.

but is typically in the neighborhood of 5–8 percent. For instance, in 2014:I (the last quarter of our sample), the QFR surveyed 8,122 manufacturing firms, out of an estimated population of 136,205. Of these surveyed firms, 3,700 had less than \$10 million in assets, 2,768 had between \$10 and \$250 million in assets, and 1,654 had more than \$250 million in assets.

Manufacturing firms that are part of the rotating random sample receive a simplified (“short”) form requiring them to report their income statement and balance sheet for the quarter. Manufacturing firms that are sampled with certainty, as well as all sampled firms in other sectors, receive a somewhat more detailed (“long”) form, which requires them to provide more information on the composition of their debt and their financial assets (US Census Bureau 2020b). Based on the underlying sampling frame, the Census Bureau then assigns sampling weights to each firm in order to generate population estimates of quantities of interest.¹⁸

B. Data Construction

The micro files of the QFR required substantial initial work in order to construct a usable panel dataset.¹⁹ This is because, in comparison to other Census datasets like the Longitudinal Business Database, the QFR microdata has almost never been used by researchers and, to our knowledge, not at all since the move to the NAICS classification, in 2000.²⁰ The Census Bureau provided raw data files from 1977:III to 2014:I, but these data files were not linked across quarters. To compute investment rates and growth rates, firms had to be linked across quarters. In general, a survey identifier was available; however, changes in the encoding format of the survey identifiers on a number of quarters required us to match firms based on other identifiers. To do so, we relied on the employer identification number (EIN) of firms, along with matches based on firm name and location of firm headquarters.

Between 1994 and 2000, the raw Census data files were missing sampling weights. We used public releases of the QFR that contain statistics of the number of firms by strata to reconstruct sampling weights over this period.²¹ These weights were also adjusted so that aggregate assets in the micro data match assets as publicly reported by the Census Bureau. Between 1977 and 1994, and post 2000, we find that, using the Census Bureau’s sampling weights, aggregate sales and assets match the publicly available releases.

¹⁸To be more precise, the QFR uses post-stratification sampling weights, which are adjusted to reflect potential changes in the composition of size and industry stratum of the firm after the stratum is formed. As a result, sampling weights may vary slightly within firm over the duration of the panel. A detailed exposition of the survey stratification and the methodology used for estimating universe totals is provided in US Census Bureau (2020a).

¹⁹An issue was that the data did not have a codebook. Because the contents of variables in the micro-data files were not always named in an unambiguous manner, it was sometimes not possible to match with certainty variables to survey response items in the short and long form. In order to deal with this issue, we matched the exact dollar values of ambiguously named variables to public reports of corporations with similar consolidation rules as those required by the QFR.

²⁰The only instance of the use of the QFR microdata of which we are aware is Bernanke, Gertler, and Gilchrist (1996), who use the pre-2000 microdata to compare firm-level to aggregate growth in sales. They do not attempt to exploit the panel dimension of the data, as we do here.

²¹Aggregates of the QFR are publicly available at <https://www.census.gov/econ/qfr/historic.html>. In a given quarter, the Census Bureau releases a set of tables by asset size class and industry; one of these tables provides the number of firms by industry and asset size class. For an example, see Table L in <http://www2.census.gov/econ/qfr/pubs/qfr09q1.pdf>.

In addition to linking the firm observations across quarters and imputing sampling weights, we also drop miscoded observations and keep only firms with strictly positive assets and balance sheet data that add up. Less than 0.1 percent of firm-quarter observations have balance sheets for which the sum of liabilities and equity does not match reported assets within less than 0.01 percent. Additionally, financial statements are consistent over time (net income equals change in retained earnings plus dividend payments) for more than 98 percent of observations, and less than 0.7 percent of observations have a zero change in sales in consecutive quarters. This suggests that the data suffers from limited misreporting, either from reporting errors or from repeated reporting of stale data. The cleaned dataset we work with contains about 1.5 million firm-quarter observations between 1977:III and 2014:I, of which about 900K are manufacturing firms.

In this paper, we focus primarily on three samples. First, the summary statistics and the time series that do not require the computation of growth rates are built off the full sample of approximately 900K firm-quarter observations for manufacturing firms. Second, we use a subsample for computing growth rates or investment rates: we require manufacturing firms to have reported data four quarters prior to the observation date, to be able to compute the year-on-year changes in quantities of interest. For the majority of small firms, which are tracked for eight quarters, taking year-on-year growth rates eliminates approximately half of the observations.²² Third, in Section VC, where we construct the cumulative responses to identified monetary policy shocks, we focus on another subsample: firm-quarter observations in manufacturing for which we have complete data for the eight subsequent quarters, so as to construct firm-level responses in the two years following the shock. Given the sample structure, this choice of window allows us to retain small firms in the analysis.²³

Additionally, in order to assess the extent to which our findings extend to other sectors, we replicate a number of key results using the sample of observations from retail and wholesale trade. The higher inclusion threshold in those sectors however means that the sample is less representative of smaller firms, so that the results we obtain there should be interpreted with caution.

C. Relationship to Other Data Sources

The QFR dataset has some advantages relative to Compustat (Standard and Poor's 2020), which is the primary firm-level dataset in use. The primary advantage is that the QFR provides a representative sample of the population of US manufacturing firms with more than \$250K in assets; the sampling frame is drawn from IRS administrative data and response is mandatory.

Relative to Compustat, the QFR asks firms for a domestic consolidation of their financial statements. For firms with significant global operations, a substantial fraction of income may be earned outside the United States and a significant

²²The growth rate sample is more than half the full sample due to the presence of large, continually sampled (long-form) firms.

²³The resulting confidential firm-level panel dataset is referenced as Crouzet and Mehrotra (2020). The disclosed time series and regression output are available with our dataset.

fraction of assets may be located outside the United States. As an input into the national accounts, the QFR attempts to more accurately measure activity within the United States. The QFR data provides somewhat more detailed information on firm assets and liabilities than what is typically available in Compustat. For example, the QFR asks firms to classify their liabilities into bank and non-bank liabilities and, for larger firms, to provide estimates of bonds and commercial paper outstanding.²⁴ Section IID provides a comparison of key summary statistics between the sample used in this paper and the Compustat manufacturing segment.

Aside from Compustat, alternative US datasets for small firms include the Survey of Small Business Finances, Orbis (Dinlersoz et al. 2018), and Sagemworks (Asker, Farre-Mensa, and Ljungqvist 2011). The most important difference between the QFR and these datasets is that it provides a longer time horizon and higher frequency of observation needed to analyze cross-sectional differences in the business-cycle behavior of firms.

Finally, and as mentioned in the introduction, the Census Bureau also releases an aggregated version of the QFR each quarter. An important challenge facing the use of these releases by researchers is that the data are tabulated by nominal asset size bins. For instance, in 2014:I, the manufacturing segment of the public release of the QFR tabulates results by groups of firms with less than \$5, \$5–10, \$10–25, \$25–50, \$50–100, \$100–250, \$250 (million)–1 billion, and more than 1 billion, respectively. These bins are changed infrequently: in particular, the list of bins described above has not changed, in nominal terms, since 1982. Because of both inflation and real growth, firms thus progressively reclassify toward higher size bins, making it more difficult to define or isolate smaller firms. With the underlying microeconomic data, on the other hand, the quantiles of the *current* distribution of book assets can be easily constructed and used to construct size groupings that do not suffer from the same reclassification issue.

D. Summary Statistics

Table 1 provides summary statistics on key real and financial characteristics for manufacturing firms. These statistics are constructed by grouping firms into quantiles of current book assets, computing moments within quantile groups, and averaging across quarters from 1977:III to 2014:I. Nominal values are deflated by the BEA price index for manufacturing, normalized to 1 in 2009:I (Bureau of Economic Analysis 2017).

Summary Statistics by Size Group.—Table 1, panel A clearly illustrates the high degree of skewness in both sales and assets. The top 0.5 percent of firms in the size distribution have assets of \$6.7 billion and sales of \$1.5 billion annually. By contrast, firms within the bottom 90 percent of the size distribution have just \$2 million in assets and \$1.2 million in sales. Investment also displays a high degree of skewness but, as Table 1 shows, investment rates are comparable across size classes so that

²⁴The QFR also require larger firms to provide a highly detailed overview of their financial assets, including, among others, cash and demand deposits inside and outside the United States and federal and local government debt owned. We do not use this data in this paper.

TABLE 1—REAL AND FINANCIAL FIRM CHARACTERISTICS, BY SIZE GROUP

Size group	0–90th	90–99th	99–99.5th	>99.5th	Compustat
<i>Panel A. Size and growth</i>					
Assets (\$ million)	2.0	48.8	626.0	6,766.3	1,797.1
Sales (\$ million, quarterly)	1.2	18.8	181.1	1,420.8	446.4
Sales growth (percent, year-on-year)	0.19	4.58	4.34	4.08	7.33
Investment rate (percent, year-on-year)	26.50	24.91	21.89	20.36	26.73
<i>Panel B. Financial characteristics</i>					
Debt to asset ratio	0.35	0.29	0.30	0.28	0.28
Cash to asset ratio	0.15	0.10	0.07	0.06	0.13
Net leverage	0.20	0.19	0.23	0.22	0.15
Short-term debt (fraction of total debt)	0.33	0.33	0.20	0.18	0.16
Bank debt (fraction of total debt)	0.48	0.57	0.43	0.28	N/A
Trade credit (fraction of total liabilities)	0.32	0.27	0.17	0.13	0.20
Intangible assets (fraction of total assets)	0.05	0.11	0.26	0.36	0.19
Zero leverage (% of total firm-quarter obs.)	20	13	8	3	4.8
Negative book equity (% of total firm-quarter obs.)	5	<1	<1	<1	1.7
Bank dependent (% of total firm-quarter obs.)	26	29	20	10	N/A

Notes: Assets and sales are averages from 1977:III to 2014:I within category expressed in real US\$2009; values are deflated using the price index for value added in manufacturing, available from the Bureau of Economic Analysis at http://bea.gov/industry/gdpbyind_data.htm. All other variables are ratios as described in the main text. *Bank dependent* indicates that more than 90 percent of the firm's outstanding debt is bank debt (firms with no debt are not classified as bank dependent). The data for the Compustat analysis is drawn from the Compustat annual files; for a description of the Compustat sample used, see online Appendix F. Annual Compustat sales are divided by 4 to obtain a quarterly value. Size groups are quantiles of the cross-sectional distribution of book assets in a given quarter; see online Appendix D for more details on their construction.

differences in investment intensity do not account for the skewness in investment. Finally, note that sales growth is substantially faster at the largest manufacturing firms over this period leading to a marked increase in concentration over the past 35 years.

Table 1, panel B provides key financial ratios by firm size categories. A standard measure of leverage—the debt to asset ratio—generally decreases across firm size categories. However, a standard measure of liquidity—the cash to asset ratio—is also highest among smaller firms. Overall, net leverage (debt less cash over assets) is fairly stable across size classes. We do find that smaller firms are more reliant on short-term debt and bank debt (as a share of total debt), consistent with the notion that their access to public capital markets is more limited than firms at the top of the size distribution. Smaller firms also have more trade credit, as a fraction of total liabilities, than larger firms.

One clear difference between large and small firms—particularly among the largest 0.5 percent of firms—is the intangible asset share. Firms in the survey report separately property, plant, and equipment (tangible assets) from other long-term assets. A high share of intangible assets likely reflects the accumulation of goodwill due to past acquisitions, so that the sharp increase in intangible asset share across size classes underscores the importance of acquisitions for growth at the very largest firms.²⁵

²⁵ Even for firms with low or zero intangible asset share, the market value of the firm may differ substantially from the book value of the firm. However, our data contains only book value of assets; for most firms in our sample, which are private, no measure of the market value is readily available.

TABLE 2—AVERAGE BALANCE SHEET, BY SIZE GROUP

Size group	0–90th	90–99th	99–99.5th	>99.5th
<i>Assets</i>				
Financial assets, incl. cash	0.149	0.099	0.074	0.055
Short-term assets				
Receivables	0.284	0.229	0.165	0.124
Inventory	0.218	0.241	0.172	0.130
Other	0.040	0.037	0.042	0.041
Long-term assets				
Net property, plant, and equipment	0.269	0.288	0.289	0.287
Other, incl. intangibles	0.050	0.106	0.259	0.362
<i>Liabilities</i>				
Debt				
Due in 1 year or less				
Bank debt	0.083	0.083	0.032	0.016
Non-bank debt	0.035	0.019	0.019	0.028
Due in more than 1 year				
Bank debt	0.107	0.111	0.110	0.072
Non-bank debt	0.123	0.079	0.141	0.179
Trade payables	0.156	0.123	0.085	0.071
Other, incl. capital leases	0.099	0.121	0.187	0.233
Equity	0.393	0.463	0.426	0.416

Notes: All numbers are expressed as fraction of total book assets. Fractions may not add up to 1 due to rounding. *Financial assets* are the sum of cash and deposits, treasury and federal agency securities, and all other financial assets. Other *Short-term assets* include prepaid expenses and income taxes receivable. *Non-bank debt* includes commercial paper, bonds, and other short- and long-term notes. Other liabilities include tax liabilities and capital leases. Definitions of the variables in terms of QFR items from survey forms 300, 201, and 200 are available from the authors on request. Size groups are quantiles of the cross-sectional distribution of book assets in a given quarter; see online Appendix D for more details on their construction.

Between versus Within Size Group Variation.—It is worth emphasizing that, despite differences across size classes in various real and financial characteristics, there remains tremendous heterogeneity *within* size classes. Table 3 provides an approximate interquartile range for sales growth, leverage, and liquidity.²⁶ For sales growth and leverage, the approximate interquartile range within size bins dwarfs the differences across size bins. The interquartile range narrows for larger size classes, but nevertheless remains substantial. It is also worth noting that a substantial fraction of firms have zero leverage; these zero-leverage firms tend to be concentrated in the bottom 90 percent of the size distribution.

Comparison with Compustat.—Table 1 also reports summary statistics for firms in the Compustat manufacturing segment.²⁷ Panel A shows that the average size of Compustat manufacturing firms is close to, but lower than, the average size of

²⁶Due to data disclosure restrictions, we provide averages above and below the median within size classes, rather than the exact twenty-fifth and seventy-fifth percentiles.

²⁷Details on the construction of the sample and the definition of balance sheet ratios in terms of Compustat variables is reported in online Appendix F.1.

TABLE 3—APPROXIMATE INTER-QUARTILE RANGES FOR SELECTED VARIABLES, BY FIRM SIZE GROUP

Size group	0–90th	90–99th	99–99.5th	>99.5th
Sales growth, <p25	–26.27%	–16.59%	–12.66%	–10.97%
Sales growth	0.19%	4.58%	4.34%	4.08%
Sales growth, >p75	26.77%	25.83%	21.41%	19.19%
Leverage, <p25	0.00	0.01	0.04	0.07
Leverage	0.35	0.29	0.30	0.28
Leverage, >p75	0.47	0.39	0.39	0.36
Liquidity, <p25	0.00	0.00	0.00	0.00
Liquidity	0.15	0.10	0.07	0.06
Liquidity, >p75	0.20	0.13	0.10	0.07

Notes: All variables are averages from 1977:III to 2014:I within size group. Leverage is defined as the ratio of debt to assets, while liquidity is defined as the ratio of cash to assets. Exact percentiles are not reported in order to preserve data confidentiality. Size groups are quantiles of the cross-sectional distribution of book assets in a given quarter; see online Appendix D for more details on their construction.

QFR manufacturing firms in the top 1 percent of the cross-sectional distribution of assets (which is approximately \$3,696 million). Quarterly sales are also somewhat lower (\$502 million versus \$801million in the top 1 percent of the QFR), though that figure includes foreign sales for Compustat firms. Finally, capital structure for Compustat firms is similar to that of the top 1 percent firms in the QFR.

III. The Cyclical Sensitivity of Small Firms

This section measures the extent to which small firms display greater cyclical sensitivity than large firms. By “greater cyclical sensitivity,” we mean that a worsening in aggregate conditions is associated with systematically bigger declines in sales and investment among small firms than among large firms.

A. Methodology

Online Appendix D describes in detail the sample selection, the size groupings, and the measures of firm-level growth which we use throughout this section. Three points are worth noting.

First, we measure the sensitivity of *firm-level* growth to aggregate conditions. We thus sort on size at the firm level, and fully control for industry effects (and, in later sections, for firm-level differences in capital structure). This is distinct from previous work on the QFR data, which was limited to measuring the growth of aggregates by nominal size bins due to the formatting of the public releases of the QFR. We discuss this and other differences of our approach with prior work using the public releases of the QFR in Section IIID.

Second, we base our size groups on *quantiles* of the *lagged* empirical distribution of *book assets*. We use *quantiles*—for example, the bottom 99 percent versus the top 1 percent—because they are immune to long-run upward size drift due to inflation and real growth. Classifying firms by their *lagged* position in the size distribution helps alleviate the cyclical dimension of reclassification, as emphasized in

Moscarini and Postel-Vinay (2012).²⁸ Finally, we use *book assets* because, among the possible measures of size in our data, it is the most stable at higher frequencies. In particular, unlike sales, it does not display substantial seasonal variation.

Third, in our baseline estimates, we measure growth among the sample of surviving firms. In particular, we do not take into account the effect of differences in the cyclical sensitivities of the rate of entry and exit of small and large firms. Our baseline results should thus be thought of as capturing the intensive margin differences between small and large firms. We discuss the impact of entry and exit on our estimates in Section IIIC.

Finally, we first report our baseline results for the sample of manufacturing firms; in Section IIIC, we extend these results to the sample of wholesale and retail trade firms.

B. Results

Sales.—Figure 1 shows the time series for the average growth rate of sales of two size groups: the bottom 99 percent (denoted by $\hat{g}_t^{(small)}$) and the top 1 percent (denoted by $\hat{g}_t^{(large)}$). Each series is the year-on-year equal-weighted average growth rate of sales among firms belonging to each of the two size groups one year prior.²⁹

The most striking feature of these two series is perhaps how closely they track each other (their sample correlation is 0.93). In particular, from 1987 to 1990, 1995 to 2000, and 2002 to 2007, it is difficult to distinguish growth rates across these groups visually. Nevertheless, there are periods of notable divergence. The two periods that stand out the most are 1982:III–1984:I (the recovery from the Volcker recessions) and 2008:III–2009:IV (the early stages of the Great Recession). In the first instance, the growth rate of small firms far outpaced that of large firms; in the second instance, it was markedly lower. The recovery of the 1990–1991 recession also features a slightly faster growth rate of small firms. Thus, even though visually the common cyclical component in small and large firms' growth stands out most, one cannot rule out that sales growth contains a size-dependent cyclical component.

A scatterplot of $\Delta \hat{g}_t \equiv \hat{g}_t^{(small)} - \hat{g}_t^{(large)}$ against year-on-year changes in real GDP shows a positive correlation. The estimated slope coefficient of the bivariate simple OLS between the two series is 0.597, with a Newey-West standard error of 0.196 (allowing for up to 8 lags). The economic interpretation of this coefficient is that, for every percentage point decline in GDP, sales decline, on average, by 0.6 percent more among small firms than they do among large firms.³⁰

²⁸ If firms tend to cross the threshold from small to large during expansions, measures of the relative growth rate of large firms using their ex post size will be biased upward.

²⁹ The specific definition of the time series reported for the small firm group is given in equation (30) of online Appendix D, for the interquartile range $(k_1, k_2) = (0, 99)$. The large firm group corresponds to $(k_1, k_2) = (99, 100)$. Unless otherwise noted, all series are deflated by the BEA's chain type price index for manufacturing value added (bea.gov/industry/gdpbyind.htm) before computing growth rates. Section IIIC further discusses results using alternative deflators.

³⁰ This correlation is robust to alternative measures of the business cycle: growth rate of overall industrial production or manufacturing IP or the change in the unemployment rate. This correlation also holds for subsamples before and after 1992 and excluding either the Volcker recovery or the Great Recession. However, the correlation becomes insignificant if both the Volcker recovery and the Great Recession are excluded.

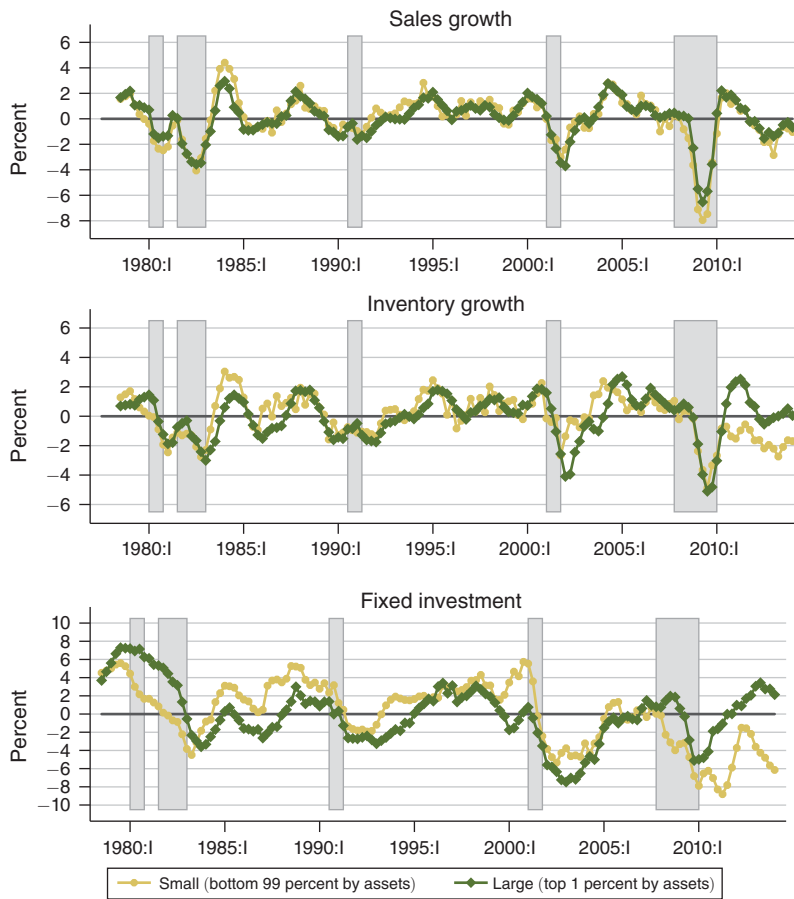


FIGURE 1. AVERAGE FIRM-LEVEL GROWTH RATES

Notes: Average firm-level growth rates of small (yellow, round markers) and large (green, diamond markers) firms; top: sales; middle: inventory growth rate; bottom: fixed investment rate time series are demeaned before plotting. Small firms are those belonging to the bottom 99 percent of the one-year lagged distribution of book assets, while large firms are those belonging to the top 1 percent of the one-year lagged distribution of book assets; see online Appendix D for more details on the construction of size groups.

Table 4 reports estimates of the semi-elasticity of firm-level growth to GDP growth. The model estimated is

$$(1) \quad g_{i,t} = \sum_{j \in \mathcal{J}} (\alpha_j + \beta_j \Delta GDP_t) \mathbf{1}_{i \in \mathcal{I}_t^{(j)}} + \sum_{l \in \mathcal{L}} (\gamma_l + \delta_l \Delta GDP_t) \mathbf{1}_{i \in \mathcal{L}} + \epsilon_{i,t}.$$

Here, i identifies a firm and t identifies a quarter. The dependent variable, $g_{i,t}$, is the year-on-year log change in sales. The set $\mathcal{I}_t^{(j)}$ is a size group; for instance, firms below the ninetieth percentile of the distribution of book assets four quarters ago.³¹ Additionally, $\Delta GDP_t = \log(GDP_t/GDP_{t-4})$ is the year-on-year growth rate of GDP,

³¹ See online Appendix D for a formal definition of the size groups.

TABLE 4—REGRESSION OF SALES GROWTH, INVENTORY GROWTH, AND FIXED INVESTMENT RATES ON GDP GROWTH FOR THE MANUFACTURING SAMPLE

	Sales growth	Inventory growth	Fixed investment
[90, 99] × GDP growth	−0.160 (0.142)	−0.107 (0.174)	−0.299 (0.157)
[99, 99.5] × GDP growth	−0.251 (0.143)	−0.299 (0.180)	−0.687 (0.194)
[99.5, 100] × GDP growth	−0.600 (0.140)	−0.730 (0.206)	−1.257 (0.355)
Observations	≈460,000	≈460,000	≈460,000
Firms	≈60,000	≈60,000	≈60,000
Adjusted R^2	0.025	0.006	0.003
Clustering	Firm	Firm	Firm
Industry controls	D/ND	D/ND	D/ND

Notes: Each line reports the estimated semi-elasticity of the variable of interest with respect to GDP growth for a size group relative to firms in the smallest size group (the [0, 90] inter-quantile range). Size groups are defined with respect to the one-year lagged cross-sectional distribution of book assets; see online Appendix D for more details on the construction of these groups. All specifications contain an indicator for durable/nondurable industries and the interaction of this indicator with GDP growth. The investment rate is computed as $(nppe_{i,t} - nppe_{i,t-4} + dep_{i,t-4,t})/nppe_{i,t-4}$, where $dep_{i,t-4,t}$ is cumulative reported depreciation between $t - 4$ and t . All values are deflated by the quarterly manufacturing price index. Standard errors reported in parentheses.

and \mathcal{L} is a set of industry dummies.³² The two main differences between this regression and the simple visual evidence are that this specifications allows for four different size groups (the bottom 90 percent, 90–99 percent, 99 percent to 99.5 percent, and the top 0.5 percent), instead of two, and that it controls for industry effects.³³

The first column of Table 4 reports estimates of the difference $\beta_j - \beta_{[0,90]}$, for the size groups $j \in \{[90, 99], [99, 99.5], [99.5, 100]\}$. For these three size groups, the difference is negative, consistent with the view that small firms are more sensitive to aggregate fluctuations. The results of Table 4 also reveal that the cross-sectional differences in cyclical sensitivity are most notable among the top 0.5 percent, which represents approximately 500 firms in each quarter. The point estimates of cyclical sensitivity decrease for the largest three size quintiles, but the difference relative to the 0–90 percent group is only statistically significant for the largest size group. It is also worth noting that the adjusted R^2 for this regression is quite low, indicating that, despite the obvious common component between small and large firms, there is considerable heterogeneity in sales growth at the firm level.

Figure 2 conveys a similar message but reports estimates of the absolute cyclical sensitivity of each size group. Specifically, it plots the average marginal effect of ΔGDP_t at the mean, for each size group (including the [0, 90] group), as well as the unconditional cyclical sensitivity (the red line). The only group with a statistically different elasticity from the unconditional cyclical sensitivity is the top 0.5 percent. Moreover, note that the absolute magnitude of the elasticities to GDP growth is substantially larger than the cross-group difference. This fact will be important in

³²The baseline regression results are reported by classifying firms into durable and nondurable industries. Section IIIC further discusses results under alternative industry classifications.

³³Section IIIC discusses the role of these industry controls.

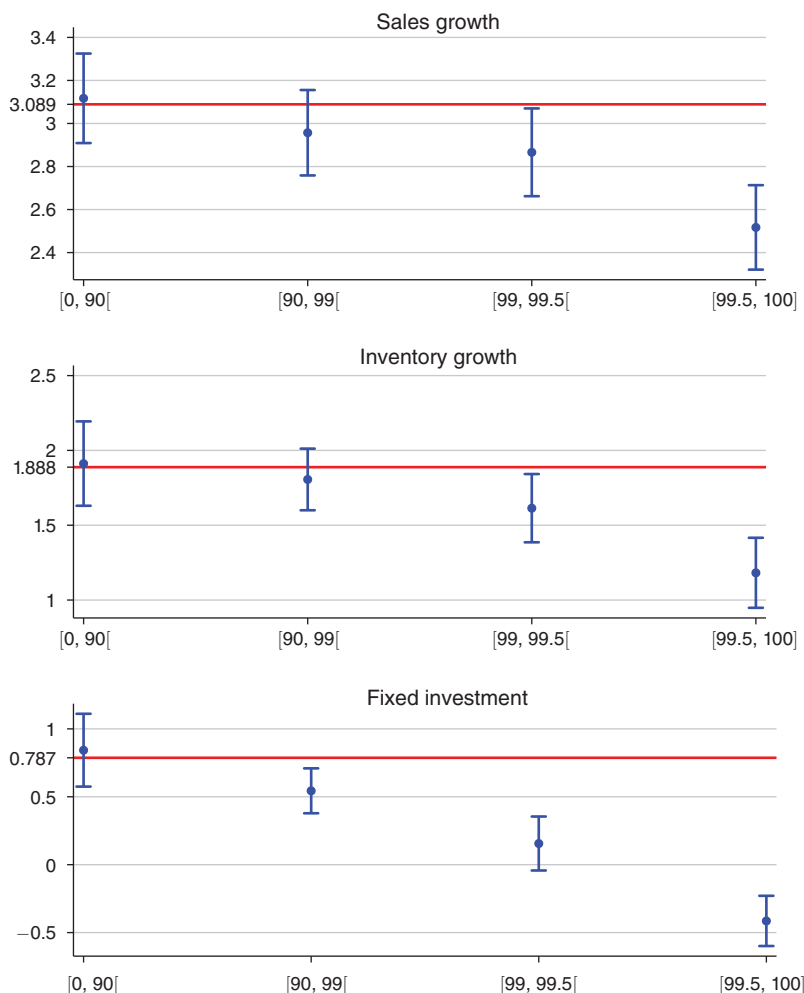


FIGURE 2. AVERAGE MARGINAL EFFECT OF GDP GROWTH ON SALES GROWTH, INVENTORY GROWTH, AND FIXED INVESTMENT, BY SIZE GROUP AND UNCONDITIONALLY

Notes: The marginal effects are computed using estimates of equation (1), whose estimation results are reported in Table 4. Blue: conditional average marginal effect by size group, with ± 2 standard error confidence interval. Red: unconditional average marginal effect.

Section IV when we consider the aggregate implications for sales of the cross-group difference in elasticities.

Investment.—The time series for inventory growth and investment in fixed assets reported in Figure 1 also displays co-movement across small and large firms but to a lesser extent than sales (the respective sample correlations between the small and large time series are 0.64 and 0.52). For inventory, the episodes of notable divergence between small and large firms are two recoveries: the 1983–1985 recovery and the aftermath from the Great Recession. These two episodes convey a mixed message. In particular, in the aftermath of the Great Recession, inventories at large firms actually recovered more quickly.

For fixed investment, the most striking fact is that contractions in fixed investment seem to occur with a lag at larger firms. This is particularly visible during the Volcker recessions. Slowdowns in investment also persist longer; in the aftermath of the 2000–2001 recession, the turning point for investment among large firms occurred approximately four quarters later for large firms than for small firms.³⁴

The regression evidence, reported in Table 4, provides a clearer picture than the long time series. The second and third columns report estimates of model (1) when the dependent variable is either inventory growth (second column) or the fixed investment rate (third column). Consistent with the behavior of sales, inventory growth of the top 0.5 percent of firms has a significantly smaller conditional elasticity to GDP growth.³⁵ The economic magnitude of the effect is large: For the bottom 90 percent, the average marginal effect of a 1 percent drop in GDP is a 1.9 percent drop in inventory, about double the effect for the top 0.5 percent.

The results for fixed investment are, if anything, starker. The difference between the 99–99.5 percent and the 99.5–100 percent groups and the bottom group are both statistically significant. In terms of economic magnitudes, a 1 percent drop in GDP is associated with a 0.9 percent drop in investment among the (0, 99) group, relative to a baseline investment rate of approximately 26.0 percent. Among the (99, 100) group, the investment drop is more muted: 0.15 percent, relative to a baseline investment rate of approximately 21 percent. The small estimated elasticity of investment to aggregate conditions among larger firms is likely driven by the fact that large firms seem to cut investment with a lag.

Nevertheless, the overall message is the same as for sales; inventory growth and investment rates among small firms are substantially more sensitive to business cycles than among large firms.

C. Robustness

Exit and Entry.—Our baseline results focus on the sample of surviving firms. This is primarily because the variables explaining nonresponse are not continuously available prior to 2000, so that we cannot confidently distinguish between true exits, corporate reorganizations, and nonresponse prior to that date. We re-estimated the size effect in the sample of all firms-quarter observations *including* unanticipated nonresponses, which account for approximately 3.5 percent of observations.³⁶ Although the point estimate for the size effect is higher including

³⁴This lag structure also accounts for the fact that the contemporaneous correlation of GDP growth and investment is not significantly positive among the largest firms in the QFR sample, as shown in Figure 2. Online Appendix F discusses this lag in more detail and shows that it is also present in both the annual and the quarterly Compustat data.

³⁵As was also the case for sales, the estimated difference in elasticities between the bottom 90 percent and the top 0.5 percent lines up with the results of a simple OLS regression of the difference in inventory growth between the top 1 percent and the bottom 99 percent, which delivers a slope coefficient of approximately 0.7. Results are not reported, but available upon request.

³⁶Note that our main results use log growth rates, which are unbounded from below and therefore not usable for exiting firms. In this computation, we instead use growth rates that are bounded from below, in order to include exiting firms. We use the bounded growth rates of Davis, Haltiwanger, and Schuh (1996), though strictly speaking, any bounded growth rate is sufficient. Online Appendix G contains a comparison of our baseline results in the continuing firm sample, which use log-growth rates, to those obtained in that same sample with DHS bounded growth rates; it shows that they are similar.

exit, it is not statistically different from the estimate excluding imputed exit. This result is driven by the fact that in this data, the imputed exit rate among the bottom 99 percent group is not substantially more volatile at business cycle frequencies than among the top 1 percent group.

Entry is poorly measured in the QFR data, because firms must have filed tax returns for at least one year in order to be included in the sample. Nevertheless, other data sources indicate that, in manufacturing, the contribution of entry and exit to overall employment growth is fairly limited. Online Appendix Figure A2 shows employment growth at all firms and at continuing firms excluding those with initial size below ten employees (this restriction is made because we estimate that the QFR does not sample firms below ten employees; however, the graph is unchanged when including all firms.) As can be seen, the differences are negligible (the correlation is equal to 0.997), indicating that employment fluctuations at continuing firms are not substantially different from overall employment fluctuations. Effectively, entry and exit do not appear to make an outsize contribution to employment fluctuations in manufacturing.³⁷

Firm Size and Firm Age.—Fort et al. (2013) argue that the business cycle behavior of firm employment depends crucially on firm age (as opposed to simply size). Firms do not explicitly report firm age in the QFR. Moreover, because of the rotating panel structure of survey, it is difficult to precisely measure it among small firms. To proxy for firm age in the QFR, we group firms (starting in 1982) into those that first appeared at least five years ago in the sample and the rest. We then re-estimate the size effect in the sample of firms at least five years of age. There are a nontrivial number of observations for small firms which are sampled in distinct periods; that is, a firm is sampled for 8–12 quarters and appears several years later re-sampled again for 8–12 quarters. This procedure has a clear drawback—firms older than five years that are only sampled once will be incorrectly classified as young. Subject to this caveat, we find that the size effect generally survives within the subsample of mature firms. Online Appendix Table A1 reports the results. For sales and for fixed investment, the size effect remains significant for both the [99,99.5] and [99.5,100] size groups. Moreover, relative to the baseline, the size effect for sales is approximately 75 percent of its baseline magnitude. For inventories, the size remains significant for the [99,99.5] size group; for top 0.5 percent of firms, it is negative, though not statistically significant. This suggests that while the size effect may be related to age, it is not solely driven by young firms (that is, it still appears among mature firms.)

The Role of Industry Controls.—In our main specification, equation (1), we control only for the industry composition of firms between the durable and nondurable industries. Moreover, size groups are defined based on the distribution of assets in the entire manufacturing sector, as opposed to within specific industries in manufacturing. Online Appendix Table A2 reports estimates of the size effect under alternative industry and size classifications. Three points are worth noting. First, without any industry controls (column 1), the size effect is more pronounced than in our

³⁷This statement should not be construed to mean that entry is not important; some subset of new firms are successful, and these firms will be sampled or will be surveyed with certainty once sufficiently large.

baseline specification. For the top 0.5 percent of firms, for instance, omitting industry controls increases the estimate of the size effect by about 25 percent. Second, narrower industry controls than the simple durable/nondurable classification (column 3) do not substantially affect estimates of the size effect. This suggests that the durable/nondurable classification is sufficient to account for the bulk of the correlation between size and industry in the manufacturing sample. Third, defining the size distribution relative to a firm's industry (either durable/nondurable, or SIC 2-digit/NAICS 3-digit) does not substantially affect the estimates of the size effect.

The Size Effect in the Trade Sector.—Because the QFR inclusion threshold is considerably lower in manufacturing (\$250 thousand) than in retail and wholesale trade (\$50 million), we chose to focus our main analysis on the manufacturing sample. However, the retail and wholesale trade samples can be used to check whether our main findings extend to those sectors. This is important in particular because trends in concentration in the manufacturing sectors (which we discuss in the following section) may have been at odds with those of other sectors. Table 5 reports estimates of the size effect among firms in the retail and wholesale trade sectors. Because of the higher inclusion threshold, we group the firms into three size bins, firms below the median by asset size, firms from in the 50–90 inter-quantile range of assets, and firms in the top 10 percent of assets.³⁸ For sales growth, inventory growth and investment, there is a significant size effect in the trade sector. The size effect is stronger for sales growth and more muted for inventory growth and investment, than in the manufacturing sector. Finally, the finding that, in the manufacturing sector, the size effect is most pronounced at the very top also holds for sales in the trade sector, and in fact to an even higher degree. The size effect is also stronger for inventory growth in the top 10 percent of firms, though the difference there is not statistically significant. Investment is the exception: there, the size effect is similar between the [50, 90] and the top 10 percent groups. Overall, the trade sector thus exhibits a size effect which, with the exception of fixed investment, is concentrated among the very top firms.

Other Robustness Checks.—Online Appendix E contains other robustness checks. Online Appendix Table A3 shows that estimates of the size effect are robust to using annual output deflators, rather than quarterly value-added deflators.³⁹ The table also shows that results are robust to controlling for industry-quarter effects. We cluster by firm on the basis that unobserved firm characteristics would be the most important factor generating correlated errors, but online Appendix Table A4 shows that results are robust to alternative clustering levels. Finally, online Appendix Figure A3 reports estimated average marginal effects of GDP growth on sales growth, inventory growth and investment (analogous to 2) for a more

³⁸ We choose this classification in order to approximate the real asset thresholds corresponding to the top 10 percent and top 1 percent in manufacturing; for instance, the top 10 percent of firms in the trade segment of the QFR sample have approximately \$2 billion in constant 2009 dollars, close to average assets in the 1 percent of the firms in the manufacturing segment.

³⁹ A complementary question is whether our finding of a size effect for sales extends to value-added, as the two may have different cyclical properties. Online Appendix E discusses this question; we thank an anonymous referee for raising this point.

TABLE 5—REGRESSION OF SALES GROWTH, INVENTORY GROWTH, AND FIXED INVESTMENT RATES ON GDP GROWTH FOR THE TRADE SAMPLE

	Sales growth	Inventory growth	Fixed investment
$[50, 90] \times \text{GDP growth}$	−0.335 (0.125)	−0.489 (0.072)	−0.442 (0.054)
$[90, 100] \times \text{GDP growth}$	−0.910 (0.000)	−0.592 (0.047)	−0.439 (0.095)
Observations	≈120,000	≈120,000	≈120,000
Firms	≈10,000	≈10,000	≈10,000
Adjusted R^2	0.017	0.005	0.004
Clustering	Firm	Firm	Firm
Industry controls	WHS/RET	WHS/RET	WHS/RET

Note: Each line reports the estimated semi-elasticity of the variable of interest with respect to GDP growth for a size group relative to firms in the smallest size group (the $[0, 50]$ size group). Because the threshold for inclusion in the trade segment of the QFR is higher, we use lower quantiles of the distribution of book assets in order to define our size groups. Size groups are defined with respect to the one-year lagged cross-sectional distribution of book assets; see online Appendix D for more details on the construction of these groups. All specifications contain an indicator for the wholesale/retail sector, and the interaction of this indicator with GDP growth. The investment rate is computed as $(nppe_{i,t} - nppe_{i,t-4} + dep_{i,t-4,t})/nppe_{i,t-4}$, where $dep_{i,t-4,t}$ is cumulative reported depreciation between $t - 4$ and t . All values are deflated by the quarterly core PCE index. Robust p-values reported in parentheses.

disaggregated size classification. The results show that the size effect is remarkably homogeneous among firms in the $[0, 90]$; with the exception of investment in the top 25 percent of firms by size, and, very marginally, sales for the $[50, 75]$ size group, the sensitivity of firms in the bottom $[0, 90]$ is in general not different from the unconditional average sensitivity. By contrast, the sensitivity of firms in the top 0.5 percent by size is systematically and significantly lower than average.

D. Discussion

This section discusses two important questions about our results so far. First, what is to be gained from using firm-level data, rather than the public releases of the QFR? Second, how do our results relate to the influential contribution of Gertler and Gilchrist (1994)—hereafter, GG?

Why Is the Micro Data Useful?—The Census Bureau publishes quarterly tabulations of the microeconomic data studied in this paper, which take the form of aggregates by bins of asset size.⁴⁰ Why use the microeconomic data, instead of these public tabulations?

The first column of Table 6 estimates the cyclicity of small and large firms using the public tabulations.⁴¹ The asset size bins used in these tabulations are fixed in nominal terms, making it challenging to consistently define small and large size

⁴⁰For instance, in 2014:I, the tabulations in manufacturing contain aggregates for firms in the \$5, \$5–10, \$10–25, \$25–50, \$50–100, \$100–250, \$250 (million)–\$1billion, and more than \$1billion asset bins, respectively. These bins change infrequently: the last change, over the sample period we study, occurred in 1981.

⁴¹We used the data available online from 1987 onwards; prior to that, we digitized paper records of the QFR.

TABLE 6—THE SIZE EFFECT ESTIMATED USING THE PUBLIC AGGREGATE TABULATIONS OF THE QFR AND FIRM-LEVEL MICRO DATA

	Elasticity to growth rate of GDP					
	GG growth rates		CM growth rates (firm-level)		CM growth rates (aggregate)	
Small firms sales growth	2.286	(0.373)	2.962	(0.362)	2.378	(0.446)
Large firms sales growth	2.564	(0.574)	2.365	(0.304)	2.278	(0.526)
Difference	-0.278	(0.359)	0.597	(0.196)	0.100	(0.287)

Notes: The first column focuses on growth rates for small and large firms constructed using the public tabulations of the QFR; the methodology follows Gertler and Gilchrist (1994) and is described in detail in online Appendix D. Each line reports the elasticity of a variable of interest (small firms' sales growth, large firms' sales growth, and their difference) with respect to GDP growth, along with Newey-West standard errors robust up to eight lags. The second and third column focus on growth rates constructed using the micro data. The second column uses the equal-weighted average firm-level growth rate among continuing firms; small firms are defined as the bottom 99 percent, and large firms as the top 1 percent. The time series used in the second column are thus identical to those plotted in Figure 1. The time series used in the third column are constructed based on the same group of continuing firms and the same size classification as column 2. However, instead of equal-weighted average firm-level growth rates, as in column 2, column 3 focuses on aggregate growth rates. Section IIID provides more detail on the comparison across data sources. The sample is 1977:III–2014:I. Standard errors shown in parentheses.

groups. The results in the first column of Table 6 use the methodology proposed by GG to address this issue. This methodology assumes that small firms account for a constant share (30 percent) of total sales.⁴² The first column of Table 6 shows that using this methodology, the size effect, measured as the semi-elasticity of the difference between small and large firm growth to GDP growth, is not statistically distinct from 0 (the point estimate is in fact *negative*.) This result is consistent with Chari, Christiano, and Kehoe (2013) and Kudlyak and Sanchez (2017), who also use the tabulations to document the cyclicity of small firms' sales does not appear to exceed that of large firms. By contrast, recall that when average small and large firm growth rates are constructed using the underlying microeconomic data and the methodology described in Section IIIA, the size effect is equal to approximately 0.6, and statistically different from 0 (the second column of Table 6 repeats this result).

There are three potential reasons for this difference. First, using the tabulations, one can only measure *aggregate* growth rates, instead of firm-level growth rates. Second, using the tabulations, one cannot condition on the initial (or lagged) size of firms; firms migrating across size categories during downturns can lower estimates of the size effect, as argued by Moscarini and Postel-Vinay (2012). Third, the methodology of GG fixes the share of small firms in total sales, whereas it might instead be decreasing. This could weaken any potential size effect over time, as more firms need to be classified as small in order to meet the 30 percent sales threshold.

While it is difficult to establish precisely the role of each factor, one can isolate the role of the first one, the difference between firm-level growth rates and aggregate growth rates. In order to do so, in column 3 of Table 6, we report the size effect estimated using the aggregate growth rate of small and large firms, where small and

⁴²The challenge posed by the public tabulations is that smaller bins tend to mechanically be occupied by fewer and fewer firms over time. For instance, the cumulative share of total sales of firms with less than \$100 million in assets in the public tabulations fell from 24 percent to 14 percent from 1990:I to 2010:I. In order to address this issue, GG suggest defining small firms as those in the smallest set of bins accounting for 30 percent of total sales in any given quarter and large firms as the remainder. This methodology is described in detail in online Appendix D.

large firms are otherwise defined as in our baseline approach.⁴³ This column shows that the magnitude of the size effect is positive, but much smaller, and statistically insignificant, when using aggregate growth rates. This suggests that value-weighting tends to dampen the relative cyclicity of the growth rate of small firms, which results in an attenuated size effect. In keeping with this intuition, Section IV discusses the relationship between the size effect estimated at the firm level, and aggregate fluctuations in sales, inventory, and investment, and generally documents a small amount of “amplification” due to the higher cyclicity of firm-level growth and investment at the firm level.

Aside from this comparison, it should be stressed that the goal of this paper is not simply to measure the size effect, but also to assess whether the higher cyclicity of small firms is related to financial frictions. The main advantage of using the microeconomic data over the public tabulations is that one can simultaneously control for size and other firm characteristics, in particular, proxies for financial constraints. This is the focus of Section V.

Comparison to Gertler and Gilchrist (1994).—GG study changes in the sales of small and large firms around the six dates identified by Romer and Romer (1989, 1994) as exogenous contractions in monetary policy. The main finding is that sales of small firms decline by 10 percent, on average, in the three years following a Romer date, while sales of large firms only decline by 5 percent (their Figure II, p. 321). Therefore, small firms are twice as sensitive to Romer episodes as large firms. By contrast, our main estimates suggest that sales of small firms are only about 24 percent ($= (3.1 - 2.5)/2.5$) more sensitive to declines in GDP than large firms.

It should be noted that GG report conditional event study responses around the six Romer dates in their sample, while we document unconditional differences in cyclicity across size groups. In particular, it may be the case that differences between small and large firms are more pronounced around monetary policy contractions. Aside from this important distinction, our analysis differs from GG in two other ways: measurement methodology, and sample period. First, as mentioned in Section IIID, GG rely on public tabulations of aggregates in order to conduct their analysis, whereas we use the underlying firm-level micro data. Second, we study a later period: GG study 1958:IV to 1991:IV, while we study 1977:III to 2014:I as the micro data is not available prior to 1977:III.

Table 7 contains summary statistics from the exercise of GG, in the column marked “GG growth rates.” We start by replicating their results, in their original sample of 1958:IV to 1991:IV.⁴⁴ We find that for large firms, the cumulative decline in sales 3 years after the Romer date is $\Delta_L = -7.0$ percent, while it is $\Delta_S = -13.1$ percent for small firms. Thus, in our replication of their exercise, small firms are 87 percent more sensitive to Romer episodes.

⁴³In other words, the only difference between the estimates of column 2 (that is, our baseline estimates) and the estimates of column 3 is the fact that column 3 uses aggregate growth rates—the underlying sample is identical. Note that these aggregate growth rates cannot be defined from public tabulations; they require knowing the entire cross-sectional distribution of firms by asset and being able to condition on one-year-lagged size.

⁴⁴In particular, after demeaning the GG growth rates and removing quarter fixed effects in order to deseasonalize them, we construct the cumulative change in sales of small and large firms around each of the six Romer dates from the original GG analysis: 1966:II, 1968:IV, 1974:II, 1978:III, 1979:IV, and 1988:IV. Online Appendix Figure A4 reports the corresponding event study plots; this figure is directly comparable to Figure II, p. 321, of GG.

TABLE 7—COMPARISON WITH GERTLER AND GILCHRIST (1994)

Sample	Average response around Romer dates (%)	
	GG growth rates	CM growth rates (firm-level)
1958:IV to 1991:IV	$\Delta L = -7.0$	N/A
	$\Delta S = -13.1$	N/A
1977:III to 1991:IV	$\Delta L = -6.8$	$\Delta L = -7.0$
	$\Delta S = -10.8$	$\Delta S = -11.3$
1977:III to 2014:I	$\Delta L = -8.4$	$\Delta L = -5.8$
	$\Delta S = -9.5$	$\Delta S = -9.2$

Notes: Each line reports results from a different sample. *GG growth rates* report results computed using the GG methodology, described in online Appendix D. *CM growth rates* report results computed the methodology of this paper: equal-weighted growth rates for firms belonging to the bottom 99 percent and the top 1 percent of the one-year lagged distribution of assets, respectively. Some cells are marked "N/A" because the underlying micro data is necessary for the construction of CM growth rates but unavailable before 1977:III. Romer dates are the dates identified by Romer and Romer (1989) as monetary policy contractions: 1966:II, 1968:IV, 1974:II, 1978:III, 1979:IV, and 1988:IV. As in Kudlyak and Sanchez (2017), we add 1994:II and 2008:III to these two dates for the 1977:III–2014:I sample. The statistics Δ_L and Δ_S are the average cumulative change in sales around the Romer dates, for large (*L*) and small (*S*) firms, respectively. Online Appendix Figures A4 and A5 report the underlying cumulative changes in sales growth around each Romer date using the GG methodology, for the 1958:IV to 1991:IV, and the 1977:III to 2014:I samples, respectively. Online Appendix Figure A6 reports the underlying cumulative changes in sales around each Romer date using the firm-level growth rates studied in this paper, for the 1977:III to 2014:I sample.

We then repeat their exercise in two other periods. First, in the period running from 1977:III to 1991:IV (the overlap between their original sample, and our sample), we find a lower gap (61 percent) between the response of small and large firms around Romer dates. In this subsample, we can compare their measurement methodology to ours. The second column of Table 7 reports results of the same event studies, using the equal-weighted, firm-level growth rates ("CM growth rates") plotted in Figure 1. With the CM growth rates, in the overlap sample, we also find substantial differences in the cumulative change in sales around Romer dates of small firms relative to large firms. This suggests that monetary policy contractions may indeed be associated with starker differences between small and large firms. Note, however, that in this sample, there are only three Romer dates: 1978:III, 1979:IV, and 1988:IV.

We finally repeat this exercise in the 1977:III–2014:I sample. In this final analysis, we include, as Romer dates, 1978:III, 1979:IV, 1988:IV, 1994:II, and 2008:III, following Kudlyak and Sanchez (2017). We find a much smaller size effect with GG growth rates (14 percent). For CM growth rates, by contrast, the size effect is similar as in the 1977:III to 1991:IV sample (58 percent). The smaller size effect for GG growth rates is due to the fact that, according to the GG measure, large firms responded *more* around 2008:III than small firms did.

Overall, the GG growth rates give an inconsistent picture of the conditional size effect; it varies across sample periods. By contrast, with CM growth rates, the conditional size effect is more stable across sample periods, with small firms responding approximately 60 percent more to the Romer episodes than large firms. This suggests, again, that measurement matters: equal-weighted (CM) growth rates tend to produce larger and more stable estimates of the size effect than value-weighted (GG) growth rates, consistent with the discussion of Section IIID.

However, it is worth emphasizing that the event study approach around Romer dates produces fragile results even using CM growth rates. Online Appendix Table A7 reports the average event study results in the 1977:III to 2014:I sample when dropping individual Romer dates. With CM growth rates, estimates for the conditional size effect ranges anywhere from 30 percent to 121 percent; the GG growth rate estimates of the conditional size effects are similarly dispersed. The fragility of the results obtained using the conditional event study approach around Romer dates motivates our analysis in Section V, where we use local projections of firm-level growth rates on identified monetary policy shocks at the quarterly frequency to investigate the size effect in response to monetary policy contractions.

IV. Aggregate Implications

This section explores whether the greater sensitivity of small firms is an important contributor to aggregate fluctuations. In order to answer this question, we provide a simple decomposition of aggregate growth into components originating from firm-level growth in different size groups. This decomposition allows us to compute counterfactuals that quantify its contribution to aggregate fluctuations.

A. A Simple Decomposition

At first glance, it seems that to answer the question of this section, one may want to use the following simple rule of thumb: the impact of small firms' greater sensitivity is equal to the product of the typical share of total sales of small firms, multiplied by the difference in the cyclicalities of small firms' sales. The results of the previous section indicate that the difference in elasticities to GDP growth between small and large firms is approximately 0.6. Assuming (for now) that small firms' share is, on average, 50 percent, one would obtain a contribution of $0.6 \times 0.50 = 30$ bps. This number would then have to be compared to the elasticity of aggregate sales to GDP, to get a sense of the contribution of the greater sensitivity of small firms to aggregate fluctuations.

This simple rule of thumb turns out to be incomplete, at least in theory. Online Appendix G shows that the growth rate G_t of any aggregate variable of interest (for instance, sales) between quarters $t - 4$ and t , among continuing firms, can be decomposed as

$$(2) \quad G_t = \hat{g}_t^{(large)} + s_{t-4}(\hat{g}_t^{(small)} - \hat{g}_t^{(large)}) + \widehat{\text{cov}}_t.$$

Here, $s_{t-4} = X_{t-4}^{(small)}/X_{t-4}$ is the initial fraction of the aggregate accounted for by small firms, and $\hat{g}_t^{(small)}$ and $\hat{g}_t^{(large)}$ are the cross-sectional average growth rates considered in the previous section.⁴⁵ The term $\hat{g}_t^{(large)} + s_{t-4}(\hat{g}_t^{(small)} - \hat{g}_t^{(large)})$ represents

⁴⁵This section analyzes a decomposition for the same log growth rates as discussed in the previous section, up to the approximation $\log(1 + x) \approx x$. Online Appendix G derives a similar decomposition for the commonly used growth rates $\tilde{g}_{i,t} = (x_{i,t} - x_{i,t-4})/((1/2)(x_{i,t-4} + x_{i,t}))$, introduced by Davis, Haltiwanger, and Schuh (1996). The online Appendix reproduces the same decomposition using these growth rates and shows that all the results of this section are unchanged.

average firm-level growth; the contribution of the product $s_{t-4}(\hat{g}_t^{(small)} - \hat{g}_t^{(large)})$ to cyclical movements in G_t is what the simple rule of thumb described above would capture.

The decomposition (2) however highlights the presence of another term, \widehat{cov}_t . The term \widehat{cov}_t is itself a weighted average of two terms:

$$(3) \quad \widehat{cov}_t = \widehat{cov}_t^{(large)} + s_{t-4}(\widehat{cov}_t^{(small)} - \widehat{cov}_t^{(large)}).$$

Each of the two terms $\widehat{cov}_t^{(small)}$ and $\widehat{cov}_t^{(large)}$ can be interpreted as the (group-specific) cross-sectional covariance between firms' initial size and their subsequent growth.⁴⁶ These terms capture the intuition that if firms that are initially large also grow faster, then aggregate growth, G_t , will tend to outpace average firm-level growth, $\hat{g}_t^{(large)} + s_{t-4}(\hat{g}_t^{(small)} - \hat{g}_t^{(large)})$. In principle, the behavior of these covariance terms may affect how one quantifies the contribution of the greater sensitivity of small firms to aggregate fluctuations. However, as the results below suggest, empirically this issue turns out to be modest.⁴⁷

B. Results

We can use this decomposition to form the following, counterfactual growth rate of aggregate sales:

$$(4) \quad G_t^{(1)} = G_t - s_{t-4}(\hat{g}_t^{(small)} - \hat{g}_t^{(large)}).$$

This time series nets out the contribution of the greater sensitivity in average growth rates among small firms, the second term of the decomposition (2). Additionally, one can net out the contribution of differences in the covariance terms in decomposition (2):

$$(5) \quad G_t^{(2)} = G_t - s_{t-4}(\hat{g}_t^{(small)} - \hat{g}_t^{(large)}) - s_{t-4}(\widehat{cov}_t^{(small)} - \widehat{cov}_t^{(large)}) \\ = G_t^{(large)},$$

thus simply obtaining the aggregate growth rate of sales among large firms.

One way to quantify the contribution of the greater sensitivity of small firms is then to compare the comovement between a business-cycle indicator and the actual growth rate of aggregate sales, G_t , to the comovement between the same business cycle indicator and either one the two counterfactual growth rates $G_t^{(1)}$ and $G_t^{(2)}$. We do this by computing estimates of the slope term in an OLS regression of G_t , $G_t^{(1)}$, and $G_t^{(2)}$ on the annual log-change in real GDP. Table 8 reports the estimated slopes

⁴⁶Specifically, $\widehat{cov}_t^{(j)} = \sum_{i \in \mathcal{I}^{(j)}} (w_{i,t-4} - (1/\#\mathcal{I}^{(j)}))(g_{i,t} - \hat{g}_t^{(j)})$, where j is small or large firms and where $w_{i,t-4}$ is the four-quarter lagged share of the total value of the variable of interest accounted for by firm i . This term is a cross-sectional covariance up to a normalizing factor.

⁴⁷Online Appendix G contains a precise decomposition of the contribution of the covariance terms and shows that it is small, whether one looks at small firms, large firms, or all firms jointly.

TABLE 8—CYCLICAL SENSITIVITIES OF AGGREGATE SALES, INVENTORY, FIXED INVESTMENT, AND TOTAL ASSETS

	Actual β	Counterfactual 1 $\beta^{(1)}$	Counterfactual 2 $\beta^{(2)}$	Counterfactual 3 $\beta^{(3)}$	Counterfactual 4 $\beta^{(4)}$
Sales	2.293 (0.342)	2.154 (0.342)	2.270 (0.366)	1.700 (0.326)	1.817 (0.351)
Inventory	0.919 (0.226)	0.719 (0.250)	0.770 (0.226)	0.589 (0.243)	0.640 (0.218)
Fixed investment	0.584 (0.145)	0.569 (0.151)	0.569 (0.148)	0.525 (0.150)	0.525 (0.147)
Total assets	0.876 (0.121)	0.787 (0.129)	0.838 (0.119)	0.703 (0.129)	0.754 (0.119)
Observations	143	143	143	143	143

Notes: Each line reports the estimated slope in regressions of the form $Z_t = \alpha + \beta \log(GDP_t/GDP_{t-4}) + \epsilon_t$. The first column reports results for $Z_t = G_t$, where G_t is the actual aggregate growth rate. The second column uses $Z_t = G_t^{(1)}$, where $G_t^{(1)}$ is a counterfactual aggregate growth rate series in which we have assumed that the average firm-level growth rate of small and large firms is equal (so that small firms do not have greater average sensitivity to business cycles than large firms). The third column uses $Z_t = G_t^{(2)}$, where $G_t^{(2)}$ is another counterfactual time series in which we have also assumed that the covariance between initial size and subsequent growth is also the same between small and large firms. Columns 4 and 5 use the same counterfactuals as columns 2 and 3 respectively, but magnify the fluctuations of small firms to ensure that small firms display twice the elasticity with respect to GDP as large firms. Heteroskedasticity robust standard errors in parentheses.

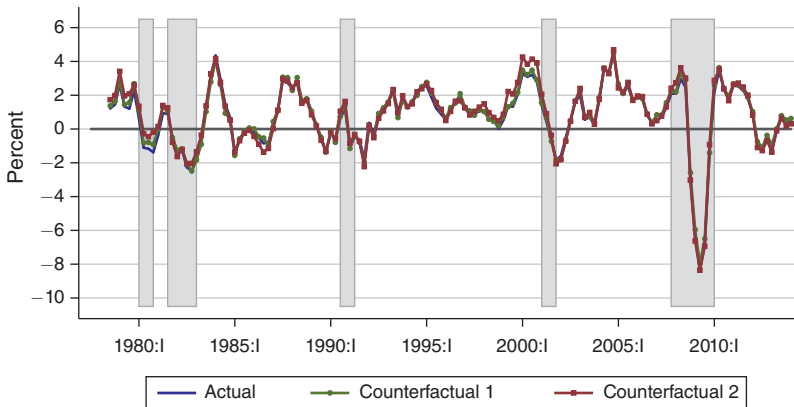


FIGURE 3. AGGREGATE GROWTH RATES

Notes: Aggregate growth rate of sales G_t (solid blue line); counterfactual growth rate $G_t^{(1)}$ (circled green line), which assumes that the average firm-level growth rate of small and large firms is equal; and counterfactual growth rate $G_t^{(2)}$ (squared red line), which also assumes that the covariance between size and growth is the same between small and large firms.

of the actual and counterfactual aggregate growth series for sales, inventory, fixed investment, and total assets. For sales (first line), the actual and counterfactual elasticities are close; the point estimates differ by approximately 13 basis points, and this difference is not statistically significant. Given the magnitude of the elasticity of aggregate sales to GDP growth (about 2.2), the economic interpretation of this difference is that, all other things equal, if the elasticity of small firms' sales growth were equal to that of large firms, aggregate sales' elasticity to GDP growth would only be about 5 percent smaller. The second counterfactual series is even closer,

indicating that cyclical variation in the difference between the covariance terms between small and large firms is, if anything, dampening aggregate fluctuations. The same conclusion holds for inventory; and it holds, in even stronger terms, for investment and for total assets.

For illustrative purposes, we consider two additional counterfactuals in Table 8. The last two columns construct counterfactuals $G_t^{(1)}$ and $G_t^{(2)}$ under the alternative assumption that small firms display twice the cyclicity of large firms. Gertler and Gilchrist (1994) documented that small firms were approximately twice as responsive as large firms after a shock to monetary policy. Counterfactuals 3 and 4 show that, if we had found differential responses of a similar magnitude, then, even given the high degree of skewness, small firms would significantly amplify fluctuations. For instance, for sales and inventories, if small firms were twice as cyclical, they would amplify aggregate fluctuations by 25 percent and 67 percent respectively.

Why are the actual aggregate growth rates, and the counterfactual growth rates that eliminate the contribution of the greater sensitivity of small firms, so close to one another? Primarily, this is due to the fact that the share of sales and investment of small firms, s_{t-4} , is very low *relative* to the difference in cyclicity between small and large firms, i.e., the term $(\hat{g}_t^{(small)} - \hat{g}_t^{(large)})$. Figure 4 reports the level (left column) and the share (right column) of total sales, inventory, fixed investment, and total assets of the bottom 99 percent of firms by size. The right column of Figure 4, in particular, corresponds to the time series s_t defined above. Two points about these time series are worth emphasizing.

First, the relative importance of the bottom 99 percent is, on average, small. Their average share of total sales, inventory, fixed investment, and total assets, are, respectively, 26.4 percent, 27.8 percent, 11.8 percent, and 16.0 percent in this sample. The particularly low share for assets reflects the extreme degree of skewness of the firm size distribution; by contrast, the fact that the share of sales is higher is consistent with the fact that smaller firms are less capital-intensive. Nevertheless, this skewness presents a first hurdle for the greater sensitivity of small firms to substantially affect aggregates.

Second, movements in the average shares seem dominated by a long-term downward trend, not business-cycle variation.⁴⁸ The share of sales of the bottom 99 percent falls from 35.6 percent in 1977:III to 20.4 percent in 2014:I, while their share of assets falls from 25.6 percent to 9.0 percent; this decline is secular over the period with an acceleration around the 2000s. This is not to say that cyclical movements in small firms' shares are completely absent: for instance, the raw correlation $\text{corr}(s_{t-4}, \Delta GDP_t)$ is approximately 0.37 in the sample. Although substantial cyclicity of the share could, in principle, offset its low average level and magnify the term $(\hat{g}_t^{(small)} - \hat{g}_t^{(large)})$, Figure 4 suggests that this is unlikely to be the case in the data.

Going back to the initial discussion of the section, the simple rule of thumb turns out to deliver an answer that are approximately correct. The results reported in Figure 4 indicate small firms' share is, on average, approximately 25 percent. The

⁴⁸ Interestingly, sales concentration in the QFR in Figure 4 exhibits two waves of increasing concentration in the early 1980s and late 1990s. The QFR is unique in offering a higher frequency measure of changes in concentration relative to the Economic Census used in Autor et al. (2017).

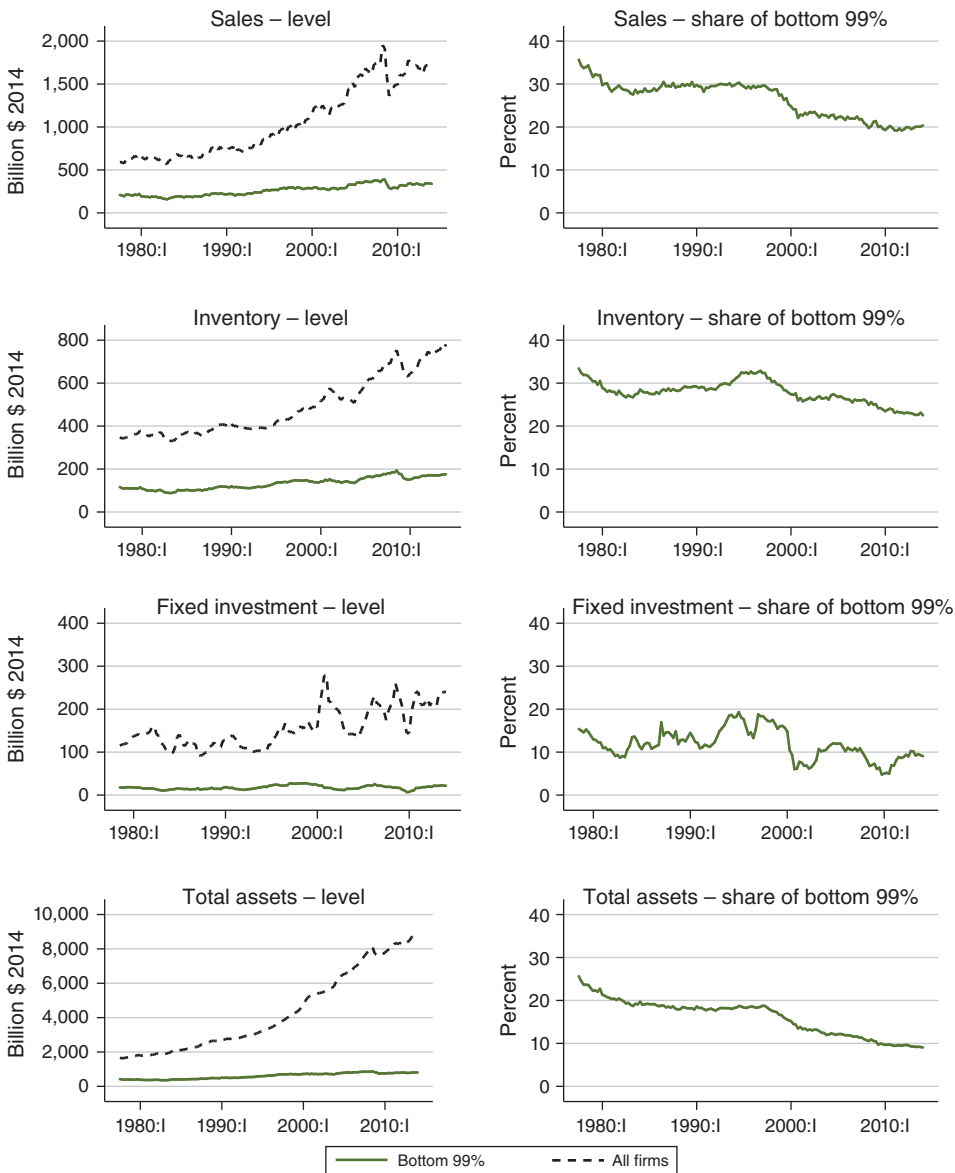


FIGURE 4. CONCENTRATION OF SALES, INVENTORY, FIXED INVESTMENT, AND TOTAL ASSETS IN THE US MANUFACTURING SECTOR

Notes: The left column reports total nominal values for the bottom 99 percent and top 1 percent of firms by size. All series are deflated by the BEA price index for manufacturing, normalized to 1 in 2009:I; the series is available at http://bea.gov/industry/gdpbyind_data.htm. Series are unfiltered. The right column reports the share of the bottom 99 percent by size (the ratio of the corresponding graph in the left column). Size is defined in reference to the current cross-sectional distribution of book assets.

product of this with the differences in cyclicity documented in the previous section is $0.6 \times 0.25 = 15$ bps, or approximately the difference between the estimated and counterfactual elasticities (13 bps). The fact that this rule of thumb delivers approximately the same result as the computation reported in Table 8 indicates that both

cyclical movements in the covariance term and cyclical variation in small firms' share, have a limited impact on the cyclical fluctuations in aggregate growth.⁴⁹

It is important to insist on two aspects of this result. First, the decomposition (2) is only correct if the set of firms entering aggregate sales is held constant from t to $t - 4$ (as is done in all the calculations of this section). Thus, the results of this section quantify the contribution of the greater sensitivity of small firms to the intensive margin of business-cycle fluctuations in aggregate sales and investment; they are silent about the extensive margin (the business-cycle fluctuations driven by entry and exit).⁵⁰ Second, these results are still consistent with the view that small firms contribute to aggregate fluctuations more than their share of sales, inventory, or investment would suggest. That is indeed necessarily the case, given the fact that their share is roughly stable (at business-cycle frequencies) and that they display more sensitivity to cycles than large firms do—that is, given that the term $(\hat{g}_t^{(small)} - \hat{g}_t^{(large)})$ is procyclical. The point of the analysis is simply to state that the *additional* fluctuations in aggregate sales that are due to this term are small relative to the overall business cycle volatility of aggregates.

Finally, online Appendix G reports results from an alternative decomposition of aggregate growth into a small firm term, a large firm term, and a reallocation component, similar to the Shimer (2012) decomposition of fluctuations in the unemployment rate. The results from this decomposition also support the view that the greater sensitivity of small firms is not large enough to significantly amplify their contribution to aggregate fluctuations, above and beyond what their average shares of sales, inventory, or investment would predict.

C. Employment

Although we have shown that the contribution of the greater sensitivity of small firms to aggregate fluctuations in sales, inventories, and investment is small, we are unable to offer a similar calculation for employment given that firms do not report employment in this survey.⁵¹ However, we can estimate the employment threshold for large firms using firm counts from the Census Bureau's Business Dynamics Statistics (BDS). There are roughly 1,000 firms in the top 1 percent of our sample. In the BDS, the top 1,000 firms in 2014 correspond to those firms with over 2,500 employees. Likewise, given that firms are only sampled if their assets exceed \$250K, we estimate that firms with approximately less than 10 employees are not sampled.⁵² In 2014, firms with over 2,500 employees account for 43 percent of manufacturing employment (only counting firms with at least 10 employees), compared

⁴⁹Figure 3 drives home this last point, by reporting the three time series G_t , $G_t^{(1)}$, and $G_t^{(2)}$ for sales. The three overlap and are visually indistinguishable.

⁵⁰See Section IIIC for a discussion of the effects of exit on our estimates. Additionally, our decomposition does not capture the potential long-run effects that declining entry during recessions may have on aggregate growth; see, for instance, Moreira (2016).

⁵¹In principle, the QFR could be linked to other Census datasets on employment such as the LBD, but current IRS and Census Bureau restrictions on the QFR do not allow this merging.

⁵²The QFR provides the total number of firms in their sampling frame, which can be compared to firm counts by employment size in the BDS. When summing firm counts from the highest to lowest bin, the number of firms in the QFR sampling frame is more than the number of firms with 10+ employees but less than the number of firms with 5+ employees.

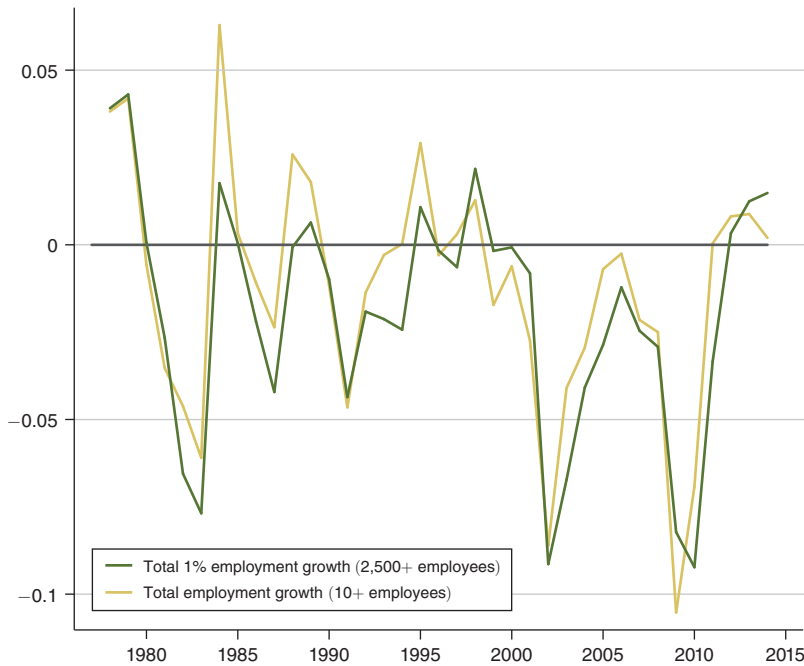


FIGURE 5

Notes: The green line displays annual employment growth for the estimated top 1 percent of manufacturing firms. The yellow line displays annual employment growth for all manufacturing firms with over ten employees (our estimate of the portion of manufacturing employment captured in our dataset).

to approximately 80 percent for sales, 76 percent for inventory, and 90 percent for investment (see Figure 4). Thus, the degree of skewness in employment is considerably less than that of sales, inventories, and investment.

Thus, to the extent that small and large firms differ in their elasticity of employment growth to GDP, these differences are more likely to be relevant for overall employment fluctuations in manufacturing. In Figure 5, we use BDS data to compute employment growth rates in manufacturing for all firms (with at least 10 employees) and for firms with more than 2,500 employees. The two series are positively correlated, but the degree of correlation is weaker than for the actual and counterfactual (excluding small firms) total sales growth series reported in Figure 3.

It is worth noting that the top 1 percent of manufacturing firms also exhibit very different trends in employment growth from small manufacturing firms. Since 1980, the share of manufacturing employment at firms with 2,500 employees has been falling over time (from about 55 percent to 43 percent in the early 1980s) with average employment growth of -1.97 percent from 1978–2014. By contrast, small firms (1–499 employees) and medium size firms (500–2,499 employees) have employment growth rates of -0.60 percent and -0.51 percent respectively. The contraction of employment at the largest firms coupled with the high average sales growth at the top firms (discussed in Section I) implies a large decrease in labor share in manufacturing. This is consistent with the evidence in Kehrig and Vincent (2017) who document reallocation of activity towards the most productive manufacturing firms which have simultaneously decreased their labor share.

V. The Financial Origins of the Cyclicity of Small Firms

As mentioned in the introduction, the early financial accelerator literature emphasized a variety of mechanisms whereby recessions, including ones not originating in the financial sector, could be worsened due to the presence of financial frictions. In this section, we investigate whether the size effect we have documented should be interpreted as evidence of such financial amplification. Here, note that the use of microeconomic data plays a central role, as it allows us to condition *simultaneously* for size and financial factors for the entire sample of firms, so as to verify whether the role of size in explaining cyclicity can be accounted for by these other factors.

We start by including various proxies for balance sheet strength in our size regressions; we find that the size effect remains significant and, in most cases, is quantitatively unchanged. However, it is possible that size is simply a better proxy for financial constraints. An additional prediction of typical financial amplification models is that small (or constrained) firms should also exhibit more cyclical financing flows than large (or unconstrained) firms. However, we show that this prediction is not borne out in the data. Finally, while the financial accelerator mechanism should, in principle, operate regardless of the underlying source of aggregate fluctuations, it may nevertheless be more potent following shocks that directly affect firms' cost of capital. In order to test this hypothesis, we explore the relative responsiveness of small firms to identified shocks to monetary policy. We find that, while the sales and investment of small firms tends to contract more than those of large firms in response to an exogenous monetary tightening, the difference is not statistically significant.

A. The Size Effect and other Proxies for Financial Constraints

We start by examining how estimates of the size effect vary when controlling for observable financial characteristics at the firm level. We start by estimating the following “horse-race” regressions in the manufacturing sample:

$$(6) \quad g_{i,t} = \sum_{j \in \mathcal{J}} (\alpha_j + \beta_j \Delta GDP_t) \mathbf{1}_{\{i \in \mathcal{I}_t^{(j)}\}} + \sum_{l \in \mathcal{L}} (\gamma_l + \delta_l \Delta GDP_t) \mathbf{1}_{\{i \in \mathcal{L}\}} \\ + \sum_{k \in \mathcal{K}} (\zeta_k + \eta_k \Delta GDP_t) \mathbf{1}_{\{i \in \mathcal{F}_t^{(k)}\}} + \epsilon_{i,t}.$$

In these regressions, the size controls are the same as in Section II; size groups, indexed by j , are defined using lagged firm size, and results for 90–99th percentile, 99th to 99.5th percentile, and top 0.5 percent are reported relative to the baseline 0–90 percent group. As before, we also include indicators for durable and nondurable manufacturing.⁵³ In contrast to the baseline regression, $k \in \mathcal{K}$ now indexes groups of our measures of financial strength. We consider five different

⁵³Our results hold when controlling for NAICS 3-digit industries.

measures of financial strength: bank dependence, leverage, liquidity, access to public debt markets, and dividend issuance. Though these balance sheet variables are endogenous (along with firm size), we view these regressions as a useful test as to whether financial factors can explain the size effect.

Column 1 in Table 9 controls for the degree of bank dependence in the size regression. Our measure of bank dependence is the share of bank debt in total debt. This variable has a bimodal distribution, with some firms nearly fully reliant on bank debt and some firms (including zero leverage firms) have no reliance on bank debt. We sort firms into low bank dependence firms (with a bank share of less than 10 percent), intermediate bank dependence firms (between 10 percent and 90 percent), and high bank dependence firms (over 90 percent).

Column 2 controls for leverage. We split the sample into four bins: firms with zero debt, firms with a debt to asset ratio of less than 15 percent, firms with a debt to asset ratio of between 15 percent and 50 percent, and firms with debt to asset ratio over 50 percent. Firms with leverage less than 15 percent approximately account for the bottom quarter of the leverage distribution, while firms above 50 percent account for approximately the top quarter.

Column 3 controls for liquidity. We consider three liquidity classes: cash to asset ratio of less than 1 percent; cash to asset ratio between 1 percent and 20 percent; cash to asset ratio above 20 percent. As with leverage, we choose fixed thresholds that approximate the bottom and top quartiles.⁵⁴

Column 4 controls for access to public debt markets. Specifically, we classify a firm-quarter observation as having access to public debt markets if the same firm has ever reported some positive liability in either commercial paper or long-term bonds. Because it relies only on responses from the long-form survey, this variable is most informative for the largest firms (it is equal to zero for firms receiving the short-firm survey). As documented by Faulkender and Petersen (2005), even among publicly traded firms, only a minority have access to public debt markets, so that there is meaningful variation in this measure among large firms.

Finally, column 5 controls for dividend issuance. A firm-quarter observation is classified as a dividend issuer if it issued dividends in the year prior to the quarter of observation. About half of firm-quarter observations in the regression sample are dividend issuers.

For bank dependence, leverage, liquidity, and dividend issuance, the coefficients on GDP interacted with size class—particularly, the top 0.5 percent—remain significant, and in magnitude, similar to the baseline regression. Thus, none of these controls changes the estimates of the size effect. The exception is market access, but the change in the size coefficient is inconsistent with the financial accelerator view. Based on the hypothesis that financial frictions amplify the response to shocks, one would expect firms with market access to have a lower degree of sensitivity to the business cycle and therefore the size effect to fall in magnitude once one controls for market access. Instead, we find that it rises, suggesting that firms with access to public debt markets are, if anything, more cyclically sensitive than other large firms. This result appears

⁵⁴The cash to asset ratio for the median firm in the QFR dataset rises starting around 2005. The top quartile of the cash to asset distribution, however, is fairly stable over time, rising only slightly toward the end of the sample. We use fixed thresholds for leverage given the absence of a time trend.

TABLE 9—REGRESSION OF SALES GROWTH ON FIRM SIZE AND PROXIES FOR FINANCIAL CONSTRAINTS (MODEL (6))

	Sales growth					
	Baseline	(1)	(2)	(3)	(4)	(5)
[90, 99] × GDP growth	−0.160 (0.260)	−0.189 (0.183)	−0.195 (0.169)	−0.162 (0.259)	−0.193 (0.179)	−0.176 (0.242)
[99, 99.5] × GDP growth	−0.251 (0.080)	−0.257 (0.075)	−0.321 (0.027)	−0.282 (0.053)	−0.490 (0.007)	−0.247 (0.140)
[99.5, 100] × GDP growth	−0.600 (0.000)	−0.563 (0.000)	−0.675 (0.000)	−0.640 (0.000)	−1.097 (0.000)	−0.594 (0.002)
Bank share [0.10, 0.90] × GDP growth		0.300 (0.158)				
Bank share < 0.10 × GDP growth		−0.315 (0.141)				
Leverage [0.15, 0.50] × GDP growth			−0.126 (0.626)			
Leverage (0, 0.15] × GDP growth			−0.474 (0.071)			
Leverage = 0 × GDP growth			−0.630 (0.046)			
Liquidity [0.01, 0.20] × GDP growth				0.228 (0.262)		
Liquidity > 0.20 × GDP growth				−0.101 (0.706)		
Market access × GDP growth					0.826 (0.020)	
Dividend issuance × GDP growth						0.087 (0.717)
Observations	≈460,000	≈460,000	≈460,000	≈460,000	≈460,000	≈460,000
Firms	≈60,000	≈60,000	≈60,000	≈60,000	≈60,000	≈60,000
Adjusted R^2	0.025	0.025	0.025	0.025	0.025	0.025
Industry controls	D/ND	D/ND	D/ND	D/ND	D/ND	D/ND
Clustering	Firm	Firm	Firm	Firm	Firm	Firm

Notes: Each column is a separate regression. All coefficients are the semi-elasticity with respect to GDP growth, relative to a baseline group. For size, the baseline group is the [0, 90] group. For the bank share, the reference group is the group of firms with more than 90 percent of bank debt, as a fraction of total debt. For leverage, the reference group is the group of firms with a ratio of debt to assets above 50 percent. For liquidity, the reference group is the group of firms with a cash to asset ratio below 1 percent. For market access, the reference group is the group of firms that have never issued a bond or commercial paper in the past. For dividend issuance, the reference group is the group of firms that have not issued dividends in the past year. Robust p -values in parentheses.

again in online Appendix C, where we estimate cyclical sensitivities by groups of proxies for financial strength; it may be due to firms with more cyclical investment opportunities choosing to tap bond markets at the beginning of recoveries.

In any case, the main message of Table 9 is that the greater sensitivity of small firms survives and is, in fact, almost unchanged (or even amplified) after controlling for the five simple proxies for financial constraints. In online Appendix H.1, we present the results from triple interaction regressions where we investigate whether the size effect differs after binning firms by financial strength (as proxied by the five ratios considered in Table 9). We find that differences in the size effect between the financially strong and weak bins is neither uniform in terms of sign nor statistically significant.

Additionally, in online Appendix H.2, we re-estimate the same specification as equation (6), but for the trade segment of the QFR, where recall that, in Section IIIC,

we documented a size effect for sales of similar magnitude as in the manufacturing segment. Online Appendix Table A15 shows that results are the same as in the manufacturing segment: namely, the greater sensitivity of small firms is unchanged after controlling for proxies for financial constraints.

Here, it is important to highlight a general limitation of these results: the simple proxies for financial constraints which we study may not adequately capture the wedge between the cost of internal and external finance, that is, the degree to which firms are financially constrained. This concern is raised, in particular, by Farre-Mensa and Ljungqvist (2016), who use shifts in the demand for credit induced by changes in the tax treatment of debt to show that, in a sample of publicly traded firms, typical proxies for financial constraints, including the more advanced constraints indexes of Whited and Wu (2006) and Hadlock and Pierce (2010) may not properly indicate whether a firm is financially constrained. While we acknowledge these limitations, it is difficult to address them without auxiliary data sources, in particular on the price of external finance, which may not be available for the sample of smaller and potentially more financially constrained firms. However, note that the goal of this paper is not to provide a new metric for the degree to which a firm is financially constrained, but rather to rule out that financial constraints explain the differential cyclicity of small firms. The following section proposes to use two tests, motivated by theory, which use ex post decisions to borrow and accumulate cash behavior (as opposed to ex ante differences in financial characteristics) to further rule out the possibility that the differential cyclicity of small firms is driven by financial factors.

B. *The Behavior of Debt*

The findings of the previous section may be driven by the fact that size is a superior proxy for financial constraints. A central idea for the financial accelerator mechanism is that the supply of external funds (typically, debt) to constrained firms should be more cyclical. A more cyclical supply of funds, in turn, should translate to a higher responsiveness of net borrowing to expansions and recessions among constrained firms. Online Appendix A illustrates this mechanism with a simple model where firm size is, by construction, a perfect indicator of financial constraints. A key prediction of the model is that greater cyclicity of investment among small firms, if it is driven by financial constraints, should also translate into greater sensitivity of debt issuance.

In order to compare this prediction to the data, we compute the cumulative change in variables of interest in a 15-quarter window around the beginning of a recession. Let $g_{i,t}$ denote one of the outcome variables of interest; we estimate the model

$$(7) \quad g_{i,t} = \alpha + \beta \mathbf{1}_{\{i \in \mathcal{I}_t^{(0,99)}\}} + \sum_{k=-4}^{10} (\alpha_k + \beta_k \mathbf{1}_{\{i \in \mathcal{I}_t^{(0,99)}\}}) \mathbf{1}_{\{t+k \in \mathcal{H}\}} + \epsilon_{i,t},$$

where $i \in \mathcal{I}_t^{(0,99)}$ is the set of small firms, defined as the bottom 99 percent of the lagged distribution of book assets, and \mathcal{H} is one of four recession start dates:

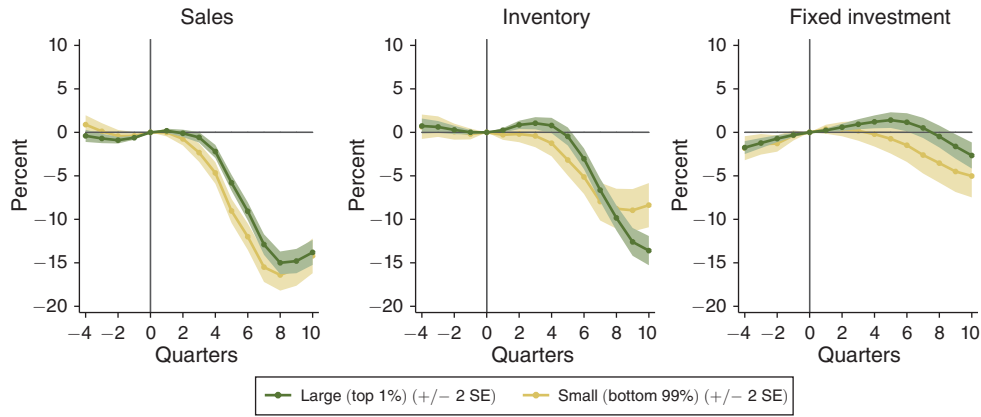


FIGURE 6. THE BEHAVIOR OF SALES, INVENTORY AND FIXED CAPITAL AFTER THE START OF A RECESSION

Notes: Each graph reports the cumulative change in a variable of interest after the beginning of a recession. Shaded areas are +/- 2 standard error bands. All growth rates are computed year-on-year and expressed at the quarterly frequency. Recession start dates are 1981:III, 1990:III, 2001:I, and 2007:IV. See Section VB for more details.

$\mathcal{H} = \{1981:III, 1990:III, 2001:I, 2007:IV\}$. We then construct cumulative responses by size: $\{c_{\mathcal{L},k}\}_{k=-4}^{10}$ and $\{c_{\mathcal{S},k}\}_{k=-4}^{10}$ for large and small firms, respectively

$$(8) \quad c_{\mathcal{L},k} = \sum_{j=-4}^k (\alpha + \alpha_j) - \sum_{j=-4}^0 (\alpha + \alpha_j),$$

$$c_{\mathcal{S},k} = c_{\mathcal{L},k} + \sum_{j=-4}^k (\beta + \beta_j) - \sum_{j=-4}^0 (\beta + \beta_j),$$

as well as the associated standard errors. Note, in particular, that in order to avoid overlapping event windows, we only consider the second of the two recession start dates of the early 1980s.

Figure 6 reports the cumulative path of sales, inventory, and fixed capital and the associated +/- 2 standard error bands for firms in the manufacturing sector. The behavior of sales is qualitatively consistent with the baseline regression: the cumulative drop in sales following the onset of the recession is substantially larger for the bottom 99 percent of firms and the difference is statistically significant. The behavior of inventory investment and fixed investment is also qualitatively consistent with the baseline regressions; however, the differences are not statistically distinct from zero across size groups except for the cumulative decline in large firms' inventory at long lags. Perhaps the most striking qualitative feature of investment behavior is that the decline of investment among large firms seems to lag that of small firms by three to four quarters.⁵⁵ This lag is not visible in the sales response.⁵⁶

⁵⁵ Aggregate fixed capital formation, in the QFR data, lags real GDP growth by three to four quarters as well: the contemporaneous correlation with year-on-year real GDP growth is 0.19, while the three-quarter lagged correlation is 0.59. This is consistent with the recession behavior documented in Figure 6, since, as discussed below, large firms account for between 80–90 percent of total fixed capital formation in the QFR data.

⁵⁶ Also in contrast to the sales response, the lack of statistical significance suggests that the greater sensitivity documented in the baseline regressions is driven by recoveries rather than recessions. This is partly visible in

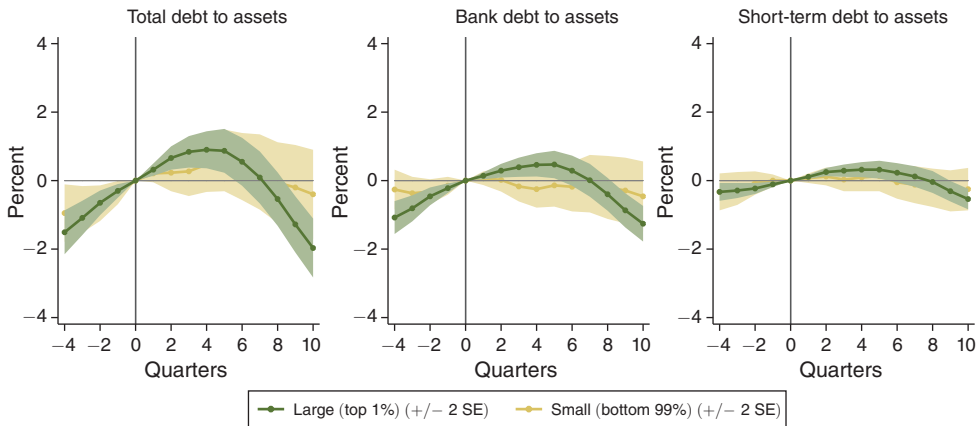


FIGURE 7. THE BEHAVIOR OF DEBT OVERALL, BANK DEBT, AND SHORT-TERM DEBT AFTER THE START OF A RECESSION

Notes: Each panel reports changes relative to quarter 0 (the recession start date), computed using the cumulative sum of average growth rate of each size group. Growth rates at the firm-level are computed as $(x_{i,t} - x_{i,t-4})/assets_{i,t-4}$, where $x \in \{\text{all debt, bank debt, short-term debt}\}$. Size groups are defined with a four-quarter lag.

Figure 7 repeats this exercise for cumulative changes in total debt, bank debt, and short-term debt. Here, short-term debt is measured as debt with maturity one year or less normalized by assets lagged four quarters, and bank debt is short and long-term bank loans normalized by assets lagged four quarters. This figure suggests that there is little difference in the cyclical behavior of debt financing at small and large firms. The left panel of Figure 7 shows that it is difficult to observe sharp differences in the behavior of debt overall. Given that the behavior of overall debt may mask significant movements in important components of debt, we also display the response of bank debt and short-term debt. The cumulative decline in bank and short-term debt is initially more pronounced among small firms, though not statistically different; eventually, the reduction in debt actually becomes bigger among large firms. The response of short-term debt among small firms is particularly, and strikingly, difficult to separate from that of large firms. Given that large firm debt contracts more than small firm debt at longer horizons, even assuming the ninety-fifth percentile responses does not deliver an economically large difference in the debt stock. For the total debt to asset ratio, small firms contract at most approximately 1.25 percent at ten quarters while large firms lowest estimated response is approximately 1 percent—a difference of only 25 basis points (the average debt to asset ratio is 30 percent).

Events studies analogous to Figures 6 and 7 for the Great Recession (not shown) display similar patterns. Small firms contracted inventories and investment (but not sales) faster than large firms. But we find no statistical difference in the rates of deleveraging. The retail and wholesale trade segments of the data behave sim-

Figure 6: the relative response of small firms' inventory at ten and more quarters out is statistically different at that stage, when recoveries are already under way. In undisclosed results, we verify that restricting the sample to the onset of recessions indeed leads to insignificant estimates of greater sensitivity for inventory and fixed capital investment.

ilarly, and we find even more muted effects comparing zero leverage and positive leverage firms. Our findings are consistent with Kudlyak and Sanchez (2017) but somewhat in tension with the findings on employment in Chodorow-Reich (2014), Duygan-Bump, Levkov, and Montoriol-Garriga (2015), and Siemer (2019). These differences may be due to the fact that (i) employment behaves quite differently than sales, investment, etc., (ii) firm credit effects may be more pronounced outside manufacturing (and the retail/wholesale firms covered by the QFR), (iii) the set of financially constrained firms identified may be only weakly correlated with firm size. It should be noted as well that we do find notable differences in the behavior of inventories and investment in the *recovery* phase of the Great Recession (see Figure 1).

In online Appendix H.2, we also conduct this exercise for the trade sample. We define small firms as those in the bottom 90 percent of the sample, and large firms as the remainder. As in the manufacturing sample, there is a statistically significant difference between the decline of sales of small firms and that of large firms around recession dates. Differences in the inventory and investment responses are even more muted, suggesting a limited role for financial amplification of investment. Additionally, while differences in the response of debt flows between small and large firms are more pronounced than in the manufacturing sector, they are not statistically significant. Overall, findings in the trade sector also seem inconsistent with the view that recessions are associated with sharper contractions in credit supply among small firms which in turn lead to larger declines in debt flows and investment.

Finally, we note that an alternative mechanism through which financial constraints may affect firms is precautionary saving by firms anticipating the possibility of being financially constrained in the future.⁵⁷ This mechanism is studied by among others, Bolton, Chen, and Wang (2014) and Abel and Panageas (2020). For instance, small firms may be more likely to see their lines of credit cut by banks during a downturn. In anticipation of this possibility, they may decide to cut investment and accumulate cash. This response would not necessarily manifest itself in a larger decline in total borrowing by small firms; smaller firms may even possibly draw down their credit lines in response to anticipated cuts. This alternative mechanism would tend to predict that cash balances of small firms are *less* cyclically sensitive than those of large firms, or that they tend to decline by less (and potentially even increase) during recessions. The evidence presented in Table A16 and Figure A11 in the online Appendix speaks to this mechanism. Online Appendix Table A16 reports estimates of the size effect for different measures of cash holdings and shows that cash holdings are *more* sensitive to aggregate conditions among smaller firms. Online Appendix Figure A11 shows that, in the same recession event study framework as the one used in Figure 6, cash balances among small firms appear to decline substantially relative to those of large firms. Thus, while the precautionary saving mechanism may be an important implication of financial constraints, it does not appear that *smaller* firms are more likely to engage in precautionary savings in response to aggregate shocks.

⁵⁷In the model of Figure 5.2, there is no idiosyncratic or aggregate risk other than exit risk, against which cash holdings are not a hedge.

C. The Greater Sensitivity of Small Firms to Monetary Policy Shocks

So far, we have presented evidence on the elasticity of firm sales to the US business cycle by firm size. One concern with these unconditional correlations is that they may mask important differences across firm size in the response to particular types of macroeconomic shocks. That is, some part of business cycle fluctuations may be driven by shocks that have a uniform effect across firm size while other shocks exhibit stronger effects across firm size. In particular, Gertler and Gilchrist (1994) focus on the response of small and large firms after monetary policy shocks as identified in Romer and Romer (1989). Arguably, monetary policy shocks impact the cost of external borrowing more directly, inducing countercyclical fluctuations in borrowing costs.⁵⁸ In turn, these episodes may provide a better test of the financial accelerator mechanism.

Estimation Framework.—To gauge the effect of monetary policy shocks, we examine the response of sales by firm size groups to the monetary shock series constructed in Romer and Romer (2004) and updated by Wieland and Yang (2020). We construct the responses by firm size group using a projection method analogous to Jordà (2005). Our specification is

$$\begin{aligned}
 (9) \quad \Delta y_{i,t,t+h} = & \sum_{j \in \mathcal{J}} \left(\alpha_j^{(h)} + \beta_j^{(h)} rr_{t-1,t} + \phi_j^{(h)}(L)X_t \right) \mathbf{1}_{\{i \in \mathcal{I}_t^{(j)}\}} \\
 & + \sum_{l \in \mathcal{L}} \left(\gamma_l^{(h)} + \delta_l^{(h)} rr_{t-1,t} \right) \mathbf{1}_{\{i \in \mathcal{L}\}} \\
 & + \sum_{j \in \mathcal{J}} \sum_{q=1}^4 \left(\mathbf{1}_{\{i \in \mathcal{I}_t^{(j)}\}} \times \mathbf{1}_{\{\mathbf{q}(t)=q\}} \right) s_{j,q}^{(h)} + \epsilon_{i,t,h}.
 \end{aligned}$$

Here, y is the log of sales (or other variable of interest), i indexes the firm, t is the quarterly date, h is horizon, \mathcal{J} are size groups, $rr_{t-1,t}$ is the shock, \mathcal{L} is industries, $\mathbf{q}(t)$ is the quarter (1 through 4) associated with date t , and X_t is a set of macroeconomic controls. We classify firms into two size groups, the (0, 99) and the (99, 100) groups. Our macro controls include unemployment, CPI, commodity prices, and the Fed funds rate allowing for two lags. Our industry groups are the durable and nondurable sectors.⁵⁹ The primary coefficient of interest is $\beta_j^{(h)}$, which is the response of sales in size group j at horizon h to the monetary policy shock $rr_{t-1,t}$.

As discussed in Romer and Romer (2004), the monetary policy shock is measured using the deviation of the implemented Fed funds rate from internal forecasts prior to the meeting date. The updated time series is monthly from 1969:1 to 2007:12. The sample stops thereafter because of the binding zero lower bound. We aggregate this time series to the quarterly frequency by taking the cumulative sum of the shock for each quarter and using the end-of-quarter monthly value. We

⁵⁸The financial accelerator mechanism works through balance sheet effects where a fall in the price of capital goods reduces firm net worth and raises borrowing costs. For instance, Cooley and Quadrini (2006) show how monetary policy shocks generate a larger fall at small relative to large firms; Khan and Thomas (2013) provide similar predictions in response to a credit shock.

⁵⁹Results are qualitatively unchanged when using NAICS 3-digit sub-sectors instead.

then use the quarterly time series from 1977:III to 2007:IV; our projection estimates thus exclude the response to monetary policy shocks that occurred during or after the Great Recession. In response to a 1 percentage point innovation to the shock, similar projection methods using aggregate data indicate that the Federal Funds rate increases by 1.9 percentage points on impact and mean-reverts back to zero within the first three quarters. The response of aggregate variables is strong and persistent: the trough in the response of industrial production is -1.1 percent (four quarters out) and the peak response of unemployment is a 0.35 percentage points (also four quarters out). The response of the CPI is slightly weaker, although it eventually declines by -0.5 percent two years out.⁶⁰

Results for Sales and Investment.—Figure 8 shows the response of sales, inventory investment, and fixed investment to the Romer and Romer shock series. Sales growth falls somewhat faster at small firms relative to large firms, consistent with our findings for the elasticity of firm sales growth with respect to the business cycle. However, the difference between sales growth at the top 1 percent and the bottom 99 percent is not statistically significant for most quarters. The evidence for a size effect is stronger for inventory growth, with small firms' inventory contracting while large firms' inventory continues to expand after the shock. In this case, the difference between the small and large firms is statistically significant. Investment rates, like sales growth, are more sensitive at small firms, but the difference is again not statistically significant.

Overall, the effect of monetary policy shocks is qualitatively consistent with the view that small firms are more sensitive, but the differences across size groups are not statistically significant for sales or investment. To avoid attrition bias (since small firms are sampled for eight quarters), we estimated the Jordà specification in firm-level data up to a horizon of only eight quarters. To obtain a longer horizon, we also estimated a specification analogous to (9) using cumulative average growth within firm-size classes instead of firm-level growth; these projections amount to pooling firm-level data by size class before estimating the effect of monetary policy shocks. Our findings are essentially unchanged.

Results for Debt Issuance.—The financial accelerator mechanism largely relies on differential balance sheet responses across firms. To the extent that size helps capture this mechanism, one should therefore expect to find a differential response in net external financing and, in particular, debt flows, in response to the identified shock. We therefore estimate the specification (9) using three additional dependent variables: the ratio of total debt to assets, the ratio of bank debt to assets, and the

⁶⁰Results for Jordà projections using aggregate data are available from the authors upon request. Note that an alternative approach would be to use the series identified using high-frequency variation in Fed Funds futures around monetary policy announcement dates, as in Bernanke and Kuttner (2005); Gürkaynak, Sack, and Swanson (2005); and Gertler and Karadi (2015). The time series for these shocks is only available from 1990:1 onwards, but does cover the Great Recession period. The results from such an analysis, also available from the authors upon request, are qualitatively consistent with those obtained using the Romer and Romer shocks, in that point estimates indicate that small firms display greater sensitivity, but are not statistically significant. However, one drawback of using these shocks is that, in a Jordà projection framework, they lead to an expansionary response of aggregates, as pointed out by Ramey (2016). This is also true in our firm-level data, where innovations to the shock series are associated with overall increases in sales, inventories, and, to a lesser extent, investment.

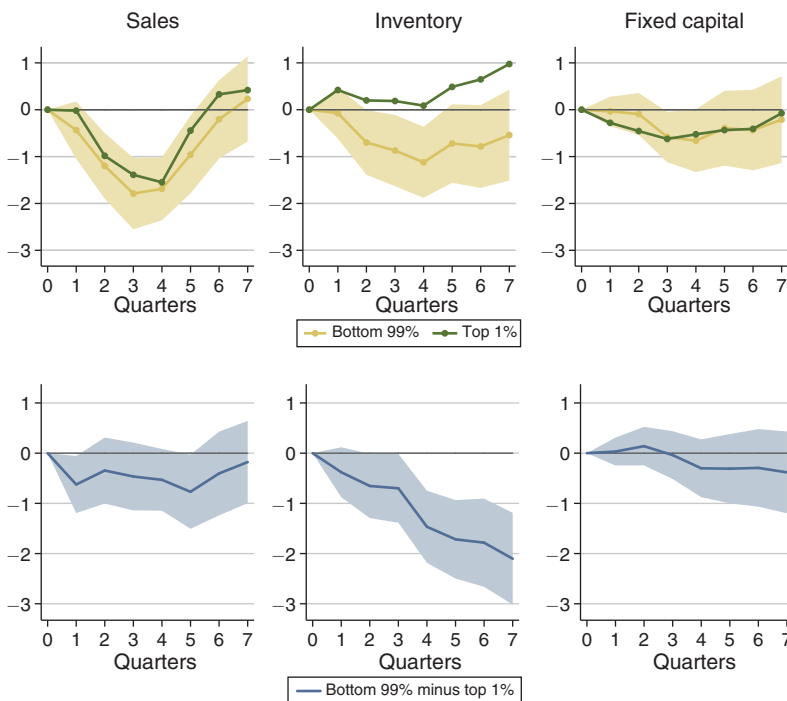


FIGURE 8. FIRM-LEVEL RESPONSE OF SALES, INVENTORY, AND FIXED CAPITAL TO AN INNOVATION TO THE ROMER AND ROMER (2004) SHOCK

Notes: The estimated specification is model (9). The top row of graphs reports the average marginal effect at the mean of a 1 percentage point increase in $rr_{t-1,t}$ for the bottom 99 percent and top 1 percent size group. The yellow shaded area is the 95 percent confidence interval; standard errors are clustered at the firm-level and heteroskedasticity-robust. The bottom row of graphs reports the difference in the OLS coefficients $\beta_{(0,99)}^{(h)} - \beta_{(99,100)}^{(h)}$, along with its 95 percent confidence interval. Data is from 1977:III to 2007:IV.

ratio of short-term debt to assets. In effect, this estimation traces out the response of firm borrowing to an identified monetary policy shock.

Figure 9 shows the cumulative change in each of these debt ratios after a exogenous tightening in monetary policy. In the case of total debt and bank debt, the point estimates show that net debt flows to small firms fall somewhat more than net debt flows to large firms at most horizons, but the difference between small and large firms is not significant. In the case of short-term debt, the response of large firms exceeds that of small firms at all horizons though, again, the difference is insignificant. The point estimates show that the differential response of debt is largest for total debt to assets; however, even at the ninety-fifth percentile, the differential effect is 75 basis points. If in response to a 1 percent monetary policy shock, large firms kept their debt to asset ratio at 30 percent (approximately the average level in our dataset), then small firms debt to asset ratio would fall to 29.25 percent indicated that the differential effect is also economically small. In comparison to the evidence at recession dates, the greater sensitivity of debt is even harder to discern, bolstering our conclusion that the size effect does not reflect the effect of financial frictions. As in Section III, we can also estimate impulse responses over longer time horizons by taking average debt growth by firm size classes and then applying the

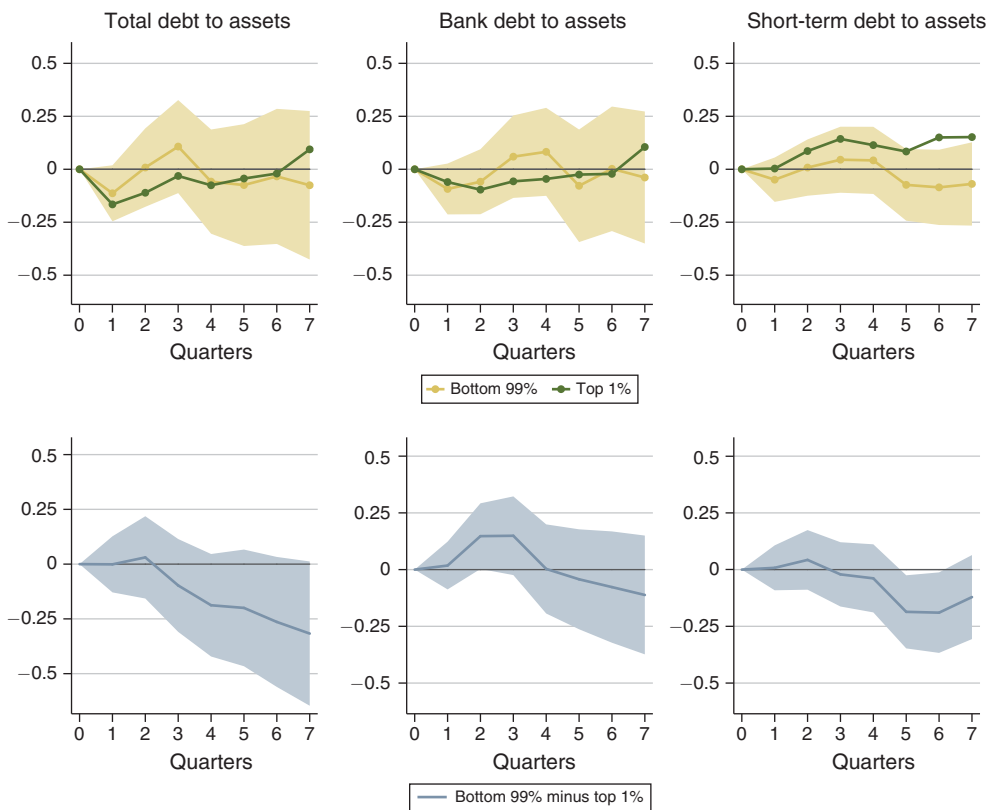


FIGURE 9. FIRM-LEVEL RESPONSE OF THE RATIOS OF TOTAL DEBT, BANK DEBT, AND SHORT-TERM DEBT TO ASSETS TO AN INNOVATION TO THE ROMER AND ROMER (2004) SHOCK

Notes: The estimated specification is model (9). The top row of graphs reports the average marginal effect at the mean of a 1 percentage point increase in $rr_{t-1,t}$, for the bottom 99 percent and top 1 percent size group. The yellow shaded area is the 95 percent confidence interval; standard errors are clustered at the firm-level and heteroskedasticity-robust. The bottom row of graphs reports the difference in the OLS coefficients $\hat{\beta}_{(0,99)}^{(h)} - \hat{\beta}_{(99,100)}^{(h)}$, along with its 95 percent confidence interval. Data is from 1977:III to 2007:IV.

Jordà method. We can also use an alternative series of monetary policy shocks from Gertler and Karadi (2015). In both cases, the point estimates are either inconsistent with the prediction that small firms are subject to tighter borrowing constraints after monetary policy shocks or the differences between small and large firms’ responses are statistically insignificant.

VI. An Alternative Mechanism for the Size Effect

This section provides an alternative interpretation of the finding that small firms are more cyclically sensitive than large firms. We document that, in the QFR sample, large firms operate across a larger range of industries than small firms. We then show that, controlling for the number of industries across which a firm operates, large firms do not appear to be less cyclically sensitive than small firms. This finding is robust to controlling for a number of factors, including the number of establishments, and holds in both the trade and manufacturing sample. Last, we provide a simple

mechanism, relying on economies of scope, to explain why multi-industry firms may be less responsive to aggregate shocks.

A. Empirical Evidence

Data Sources.—The QFR is a firm-level survey; it contains no information on the establishment composition of firms. In order to construct a measure of the establishment composition of firms, we merged the QFR to Dun and Bradstreet's Marketing Information files (Dun and Bradstreet 2019).⁶¹ This dataset, which is publicly available, contains annual establishment level data since 1990. Crucially for our purposes, the dataset records corporate linkages across establishments allowing us to construct, in each year, the list of establishments belonging to a particular firm.

The DMI annual files are the result of a large-scale data collection effort by Dun and Bradstreet. While the coverage of DMI is broad, the data has not yet been systematically assessed against sources based on administrative records. In particular, Haltiwanger, Jarmin, and Miranda (2013) discuss the potential for measurement error in sales and employment growth rates because of the high rate of imputation. However, Barnatchez, Crane and Decker (2017) argue that applying certain sample restrictions—in particular, eliminating establishments with very low or no employment—leads to high correlations in the distributions for the level of employment at the industry and zip code level between DMI and the LBD or the Quarterly Census of Employment and Wages.

Following this work, we apply systematic filters to the DMI data, excluding establishments organized as sole proprietorships, which are outside of the QFR scope and limiting the sample to establishments with a minimum of five employees. Additionally, we do not use the DMI data for measuring *growth rates* of employment. We only use the *level* of the employment measure to verify that it is positively correlated to our QFR measure of size and the *level* of the sales to compute a measure of sales concentration in manufacturing and trade within each firm. The merge is performed in two steps. The first step is to aggregate the DMI data to the firm level using the headquarters Duns number as the identifier for the firm. The second is to merge the resulting corporate entity to firms in the QFR, using the name, address, and industry of each entity in both QFR and the DMI files as merging variables. This merge is conducted separately in each year and data is available only after 1990. For this sample, we find matches for between 65 percent to 80 percent of firms in the QFR depending on the year.

Findings.—Table 10 reports summary statistics for the merged DMI/QFR manufacturing sample. The first column shows that average employment rises sharply across QFR size groups: average employment among the bottom 90 percent of matched firms is 59, whereas it is 9,649 in the top 0.5 percent of firms. The second column reports establishment counts by firm and shows that they also sharply rise with the QFR measure of size: firms in the bottom 90 percent of matched firms have 2 establishment on average while the top 0.5 percent have 67.

⁶¹For more information on Dun and Bradstreet's Marketing Information Files, see also Neumark, Wall, and Zhang (2011), Walls (2013), Barnatchez, Crane, and Decker (2017), Crane and Decker (2019).

TABLE 10—SUMMARY STATISTICS FOR THE QFR MANUFACTURING SAMPLE MERGED TO THE DMI DATABASE

	Employment (000s)	Establishments (00s)	Lines of business (00s)	Manufacturing index
[0, 90]	0.059 (0.002)	0.020 (0.001)	0.013 (0.001)	0.984 (0.001)
[90, 99]	0.404 (0.007)	0.051 (0.001)	0.031 (0.001)	0.926 (0.002)
[99, 99.5]	1.808 (0.056)	0.170 (0.007)	0.084 (0.002)	0.857 (0.005)
[99.5, 100]	9.649 (0.961)	0.667 (0.065)	0.233 (0.013)	0.818 (0.006)
Observations	≈200,000	≈200,000	≈200,000	≈200,000
Firms	≈30,000	≈30,000	≈30,000	≈30,000
Adjusted R^2	0.137	0.153	0.379	0.986

Notes: Each line corresponds to a different size group; size groups are defined based on the cross-sectional distribution of book assets, as described in Section IIIA. Each column reports the coefficients in a regression of a particular outcome variable on a full set of dummies, excluding the constant; that is, they are the conditional mean of each outcome variable by size group in the matched sample. The numbers in parentheses are standard errors, clustered at the firm level. The first column reports the conditional mean for employment (in thousands). The second column reports the conditional mean of the number of establishments (in hundreds). The third column reports the conditional mean of the number of lines of business (that is, the distinct number of SIC-4 digit codes in the collection of all establishments belonging to a particular firm; in hundreds). The fourth column reports the conditional mean of the manufacturing index, defined as the percentage of total firm revenue generated by establishments with an SIC-4 digit code in manufacturing, where revenue is measured using the DMI files.

The third column of Table 10 reports our measure of the number of industries across which a firm operates (the column marked “Lines of business”). In DMI, a primary industrial classification is reported for each establishment.⁶² We define the number of lines of business as the number of distinct primary industry classifications of establishments within the firm. Unless the firm is single-establishment, we do not include establishments identified as headquarter locations in our counts. The third column of Table 10 shows that this measure of the industry composition of firms varies with QFR size. The largest QFR firms have on average 23 lines of business while firms in the bottom 90 percent only have one. Note also that this measure is distinct from the number of establishments; in a simple regression of the number of lines of business on the number of establishments in the matched sample, the R^2 is 0.40; and their sample correlation is 0.62.⁶³ The final two columns of Table 10 also underscore differences in the industry composition of firms across size. The column marked “manufacturing index” reports the correlation between the percentage of total firm revenue generated by establishments classified as manufacturing within the firm (where establishment revenue is measured using the establishment revenue

⁶²In DMI, the establishment classification consists of an 8-digit SIC code, with the first four digits corresponding to standard SIC-4 codes and the remaining 4 digits corresponding to a proprietary industry classification by Dun and Bradstreet; we keep only the first four. Moreover, for each establishment, the DMI files may contain several industry classification codes if the establishment engages in multiple activities contributing more than 10 percent of its revenue. We only consider the primary industry classifier meaning that our measure of industry composition of establishments at the firm level is likely to understate the diversity of economic activity within firm.

⁶³For the trade sample, which we discuss below, the R^2 is 0.20 and the correlation is 0.44.

figures DMI files). The largest firms appear to generate a nontrivial portion of their revenue (18.2 percent) in establishments not classified in manufacturing, while firm in the bottom 90 percent by book assets do not.⁶⁴

Table 11 then studies how the size effect documented in Section III relates to our measure of industry scope. The first column of Table 11 establishes that there is a size effect in the matched sample; it is somewhat larger than in the baseline QFR sample, though less precisely estimated. The second column of Table 11 shows that firms with a higher number of lines of business are also substantially less cyclically sensitive. The third column shows that, after jointly controlling for size and the industry scope of the firm, the size effect vanishes. By contrast, the point estimate on lines of business is roughly unchanged. There are no significant differences in the cyclicity of small and large firms after accounting for the fact that some large firms have more lines of business. Finally, the last column also controls for the number of establishments; the effects of the number of line of business survives and is in fact strengthened, indicating that there is independent variation between these two measures of firm establishment characteristics.

Table 12 reports the results of similar regressions for the retail and wholesale trade portion of the QFR sample. From the standpoint of industry composition, matched firms tend to have more establishments and operate across fewer industries than in manufacturing (Table A18 in the online Appendix reports summary statistics). Nevertheless, Table 12 shows that the finding of lower cyclicity at large firms disappears once one controls for the industry scope; our basic finding extends to the retail and wholesale trade segment. Additionally, Tables A18 and A19 in online Appendix I show that the same findings holds if one uses continuous measures for the number of lines of business and the number of establishments rather than the groups used in Tables 11 and 12.

Overall, these findings indicate that the QFR measure of size is correlated with measures of the number of industries across which a firm's establishments operate. Moreover, this difference seems to help account, empirically, for the fact that larger firms are less sensitive to aggregate fluctuations.

B. A Potential Mechanism: Economies of Scope

We propose a simple model to rationalize the two empirical findings above: (i) large firms tend to operate across a higher number of industries than small firms; (ii) multi-industry firms are less sensitive to aggregate shocks than single-industry firms. Our model relies on two key ingredients: non-homothetic demand for firms' products and economies of scope across industries. Here, we only describe the key results and underlying mechanisms; online Appendix B reports a precise statement of the model and a derivation of these results.⁶⁵

⁶⁴We verified that these differences remain significant after controlling for SIC-2 digit/NAICS 3-digit industry fixed effects.

⁶⁵Our model relates to the literature on conglomerates (years). Both the basic facts and the supporting theories in this literature are not settled (see Maksimovic and Phillips 2013 for a review). In particular, some theories explain the behavior of conglomerates using frictionless models of the firm (Maksimovic and Phillips 2002), while others argue conglomerates may face advantages because of internal capital markets (Stein 1997). The size effect seems difficult to reconcile with internal capital market theories for three reasons: First, our evidence shows that industry scope explains the size effect, not the number of establishments; internal capital market mechanisms could

TABLE 11—THE SIZE EFFECT AND THE ESTABLISHMENT COMPOSITION OF FIRMS IN THE QFR MANUFACTURING SAMPLE

	Sales growth			
	(1)	(2)	(3)	(4)
[90, 99] × GDP growth	−0.222 (0.261)		−0.138 (0.302)	−0.255 (0.315)
[99, 99.5] × GDP growth	−0.221 (0.276)		−0.033 (0.363)	−0.259 (0.387)
[99.5, 100] × GDP growth	−0.802 (0.247)		0.048 (0.392)	−0.082 (0.412)
Lines of business ∈ [2, 5] × GDP growth		−0.426 (0.374)	−0.387 (0.340)	−0.548 (0.420)
Lines of business ∈ [5, 20] × GDP growth		−0.576 (0.389)	−0.507 (0.445)	−0.807 (0.568)
Lines of business > 20 × GDP growth		−0.900 (0.420)	−0.935 (0.418)	−1.329 (0.599)
Establishments ∈ [2, 5] × GDP growth				0.592 (0.502)
Establishments ∈ [5, 50] × GDP growth				0.527 (0.569)
Establishments > 50 × GDP growth				0.706 (0.641)
Observations	≈200,000	≈200,000	≈200,000	≈200,000
Firms	≈30,000	≈30,000	≈30,000	≈30,000
Adjusted R^2	0.033	0.031	0.034	0.035
Industry controls	D/ND	D/ND	D/ND	D/ND
Clustering	Firm-level	Firm-level	Firm-level	Firm-level

Notes: The dependent variable in all specifications is sales growth. The first three lines report the estimated sensitivity of firm-level sales growth to GDP growth for different size groups relative to a baseline category, when size is defined in terms of quantiles of book assets, as in Section IIIA; the baseline category is the group of firm in the [0, 90] inter-quantile range for size. The fourth to sixth lines report the estimated sensitivity of firm-level sales growth to GDP growth for groups of firms with different numbers of lines of business, relative to a baseline category. A firm's number of lines of business is the total number of distinct SIC-4 digit codes of the collection of establishments that make up the firm in a given quarter. The baseline category are firms with one line of business. The seventh to ninth lines report the estimated sensitivity of firm-level sales growth to GDP growth for groups of firms with different numbers of establishments, relative to a baseline category; the baseline category are single-establishment firms. Establishment counts and the industry composition of establishments for a given firm are obtained by merging the QFR with DMI. Standard errors clustered at the firm level reported in parentheses.

The model is static. A representative household is endowed with an amount I of a numeraire good and derives utility from a constant elasticity of substitution (CES) aggregate of goods produced by a set of N industries. Each industry's good is itself a CES aggregate of two goods. The first good is produced by single-industry firms (S -firms), which operate only within the industry. The second good is produced by multi-industry firms (M -firms), which operate across all N industries. All firms are price-takers.

in principle apply to firms with a large number of establishments even within an industry. Second, our evidence shows that industry scope is associated with slower growth of multi-industry firms during expansions, while internal capital market theories typically only focus on their buffering role in a downturn, particularly financially driven ones (Matvos and Seru 2014). Third, our evidence on the absence of a differential response of debt between small and large firms in Section V would seem inconsistent with an internal capital markets mechanism where financing is extended for one segment to the parent firm.

TABLE 12—THE SIZE EFFECT AND THE ESTABLISHMENT COMPOSITION OF FIRMS IN THE QFR TRADE SAMPLE

	Sales growth			
	(1)	(2)	(3)	(4)
[50, 90] × GDP growth	−0.202 (0.269)		−0.153 (0.267)	−0.114 (0.266)
[90, 100] × GDP growth	−0.669 (0.295)		−0.444 (0.298)	−0.350 (0.298)
Lines of business ∈ [5, 20] × GDP growth		−0.203 (0.231)	−0.134 (0.228)	−0.094 (0.246)
Lines of business > 20 × GDP growth		−1.270 (0.318)	−1.042 (0.324)	−0.657 (0.366)
Establishments ∈ [10, 100] × GDP growth				−0.493 (0.269)
Establishments > 100 × GDP growth				−0.613 (0.353)
Observations	≈80,000	≈80,000	≈80,000	≈80,000
Firms	≈10,000	≈10,000	≈10,000	≈10,000
Adjusted R^2	0.021	0.021	0.021	0.022
Industry controls	WHS/RET	WHS/RET	WHS/RET	WHS/RET
Clustering	Firm-level	Firm-level	Firm-level	Firm-level

Notes: The dependent variable in all specifications is sales growth. The first three lines report the estimated sensitivity of firm-level sales growth to GDP growth for different size groups relative to a baseline category, when size is defined in terms of quantiles of book assets, as in Section IIIA; the baseline category is the group of firm in the [0, 50] inter-quantile range for size. The fourth to fifth line report the estimated sensitivity of firm-level sales growth to GDP growth for groups of firms with different numbers of lines of business, relative to a baseline category. A firm's number of lines of business is the total number of distinct SIC-4 digit codes of the collection of establishments that make up the firm in a given quarter. The baseline category are firms with 1 to 5 lines of business. The sixth and seventh lines report the estimated sensitivity of firm-level sales growth to GDP growth for groups of firms with different numbers of establishments, relative to a baseline category; the baseline category are firms with less than ten establishments. Establishment counts and the industry composition of establishments for a given firm are obtained by merging the QFR with DMI. Standard errors clustered at the firm level reported in parentheses.

The first departure from standard models is that we assume that the household has non-homothetic preferences for each good. Specifically, the CES aggregate defining the consumption good of industry n is given by

$$(10) \quad C_n = \left((C_{n,S} - C_{n,S}^*)^\epsilon + (C_{n,M} - C_{n,M}^*)^\epsilon \right)^{\frac{1}{\epsilon}}, \quad \epsilon \in [0, 1],$$

where $C_{n,x}$, $x = N, S$, represent purchases of the good by the household and $C_{n,x}^*$, $x = N, S$, represents the inelastic part of the demand for the product of firms of type x in industry n . This preference specification is analogous to Geary (1950) and Stone (1954). In the macro literature, it has recently be used by Ravn, Schmitt-Grohé, and Uribe (2006) and Ravn, Schmitt-Grohé, and Uribe (2008) among others. Households take $C_{n,x}^*$ as given; it can be thought of as a subsistence level of consumption of each good, or, more broadly, customer capital accrued by firms of type x in industry n .

The second nonstandard feature is that we allow firms to invest in order to increase the inelastic component of their demand. Specifically, we assume that, subject to a convex cost $\gamma(\cdot)$, firms can raise the inelastic component of their demand. Production is otherwise standard. Firms use a single input with fixed price MC^{-1} and have constant returns to scale. Crucially, we assume that M -firms enjoy economies of scope in making these investments. Formally, we follow the definition of

Panzar and Willig (1981) and Tirole (1988), and assume that the total cost of investing, $\Gamma(\cdot)$, is subadditive. Specifically, we assume that total investment costs for a firm of type M are given by

$$(11) \quad \Gamma(\{C_{n,M}^*\}_{n=1}^N) = \left(\sum_{n=1}^N \gamma(C_{n,M})^\alpha \right)^{\frac{1}{\alpha}} \leq \sum_{n=1}^N \gamma(C_{n,M}), \quad \alpha \geq 1.$$

The parameter α controls the strength of the economies of scope; when $\alpha > 1$, the inequality above holds strictly and there are economies of scope. One interpretation of economies of scope is that firms that invest in customer capital benefit multiple products at once. For example, a multi-industry firm like General Electric advertises in terms of a general brand (GE), thereby building customer capital across all its products (jet engines, appliances, etc.).

RESULT 1: *When there are no economies of scope ($\alpha = 1$), the S and the M firms produce identical amounts in a given industry ($C_{n,S} = C_{n,M}$). Moreover, the semi-elasticity of their total sales to a shock to household income I is the same. When there are economies of scope ($\alpha > 1$), M firms produce more than S firms in a given industry ($C_{n,S} < C_{n,M}$). Moreover, the semi-elasticity of total sales of M -firms in industry n is lower than that S -firms: $\partial \log(P_{n,M} C_{n,M}) / \partial \log(I) < \partial \log(P_{n,S} C_{n,S}) / \partial \log(I)$.*

The intuition for this result is straightforward. First, in the absence of economies of scope ($\alpha = 1$), within a particular industry, S and M firms are identical. In particular, they produce the same amounts and their sales respond in the same way to a shock to either marginal cost or household income. Note that, had we assumed a standard CES demand system with $C_{n,S}^* = C_{n,M}^* = 0$, a shock to household income, for instance, would have entirely passed through to industry sales. With investment in customer capital, the pass-through is imperfect; that is, a 1 percent decline in income translates to a less than 1 percent decline in sales in each industry.

Economies of scope ($\alpha > 1$) introduces asymmetries across M and S firms within each industry. Effectively, with economies of scope, in an otherwise symmetric equilibrium, M firms face a uniformly lower marginal cost of investing in customer capital (though it is still convex). As a result, they choose to invest more in customer capital in equilibrium. Their prices are also lower, so that their overall demand is higher. Moreover, the higher investment in customer capital lowers the overall elasticity of their demand to household income shocks. Note that both S and M -firms invest in customer capital, in equilibrium; so the pass-through of household income shocks is not perfect for any firm. However, it is weaker for the M -firm, due to the economies of scope.

VII. Conclusion

This paper brings new evidence to bear on the cyclicity of small and large firms using novel firm-level data for covering a large sample of US public and private firms from 1977 to 2014. We provide strong evidence that small firms are more sensitive: a 1 percent drop in GDP is associated with a 2.5 percent drop in sales for the top 1 percent of firms by size but a 3.1 percent contraction for the bottom 99 percent.

This difference is statistically significant, holds across the trade and manufacturing sectors, holds for recession dates, and survives a battery of robustness checks. This modest difference in sensitivity combined with the high and rising concentration of sales and investment at the top 1 percent imply that small firms only have a negligible effect on aggregate fluctuations.

We provide evidence that suggests this size effect is not driven by access to financing. In particular, the size effect we document appears to be largely orthogonal to balance sheet proxies for financial strength and does not extend to debt flows which appear to be equally cyclical among small and large firms. We find only small differences in the response of sales, investment, and inventories to monetary policy shocks. Instead, we offer evidence for a nonfinancial explanation for the cyclicity of small and large firms based on differential investment in customer capital.

Our results challenge the commonly accepted view that small firms are more cyclical because of financial frictions. Additionally, they shed light on models of financial frictions where most *ex ante* firm heterogeneity is driven by net worth and financing constraints are strongly procyclical, since these models will tend to predict that size is a good proxy for the tightness of financial constraints. Finally, our results caution against using differential responses by size as a way to diagnose the financial effects of aggregate shocks, a common practice in the empirical macro and corporate finance literature. Aside from these positive implications, our results are potentially relevant for countercyclical policies supporting small business credit; they suggest that their impact may be more limited than commonly assumed.⁶⁶

As we emphasize, our results do not imply that financial frictions are generally irrelevant to the transmission of shocks to firms or over the firm life cycle. Our point is more specific: comparing the business cycle sensitivity of small and large firms is unlikely to be informative about financial amplification mechanisms. Other proxies for financial strength might well be. In this vein, online Appendix C compares the behavior in sales and investment in firms sorted by some such proxies. These proxies show little power in predicting heterogeneous responses of firms in recessions with the notable exception of dividend issuance. While their lack of predictive power may be a challenge for certain models of financial amplification, they do not rule out others—in particular, those where borrowing capacity are limited by future cash flows, instead of assets in place or net worth. Recent work by Chodorow-Reich and Falato (2017) and Lian and Ma (2019) may be more promising in thinking about the channels through which financial constraints operate in practice. We think the QFR can be useful in testing these alternative views of financial amplification in the future.

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⁶⁶ Credit support programs for SMEs are common in advanced economies; examples include the SBA's funding programs in the United States, the Small Business Financing program in Canada, the Business Finance Partnership in the United Kingdom, the European Investment Bank's small business support programs in the European Union, among others. For a discussion of these programs during the Great Recession, see Wehinger (2014).

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